Essays in Development and Labor Economics

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ABSTRACT

This thesis consists of three essays in development and labor economics. The first essay analyzes the medium-term effects of children's exposure during early-life stages of development to ENSO-related extreme rain events. Exposure to negative weather conditions is exogenously identified using geographical variation of rainfall deviations from historical averages. The main findings show that four to five years after the shock occurrence, affected children exhibit lower performance in cognitive tests and are impaired in terms of physical development with respect to same-aged children not affected by the shock. Negative effects of weather shocks on income, food consumption, and diet composition during early childhood appear to be key mechanisms behind the impacts on children's outcomes. No mitigation effects from the provision of *Progresa*, a Mexican conditional cash transfer program (CCT), are found.

The second essay follows up by analyzing the direct effect of *Progresa* on early child development. Disadvantaged early life conditions might jeopardize the later benefits that CCTs usually promote on children, mainly human capital investments. Three empirical exercises estimate the effect of *Progresa* on early life development. No effects are found on cognitive, physical, motor skill and behavioral development of children aged 2-6 that were benefited by the program during critical stages of development. Given the considerable lag of these children's initial development, the result raises concerns about *Progresa's* long-term effectiveness on poverty and inequality reductions.

Finally, the third essay empirically examines the educational selectivity of U.S. immigrants and of those that return to their source country. Ten countries are selected based on their historical importance on U.S. migration. To determine selectivity of recently arrived immigrants, their schooling distribution is compared to that of their source country. Return migration selectivity is inferred from changes in the schooling distribution of synthetic cohorts through census years. The results indicate a positive selection of immigrants, except for contemporaneous Mexican immigration. Evidence from past decades indicates that return migration accentuated the positive selection of staying immigrants, but recently, this trend has declined. No evidence of negative selection of immigrants that arrived and stayed in the U.S. is found.

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1. EL NIÑO AND MEXICAN CHILDREN: MEDIUM-TERM EFFECTS OF EARLY-LIFE WEATHER SHOCKS ON COGNITIVE AND HEALTH OUTCOMES[2]

1.1 Introduction

In rural, rain-fed agricultural settings, rainfall shocks are often cited as the most important risk factor faced by households (Mexican Ministry of Development, 1999; Fafchamps et al., 1998; Gine et al., 2010). Young children and pregnant women represent particularly sensitive populations to events of this nature. The idea that stimuli or stressful conditions during critical periods in early life can have lifetime consequences is well established in developmental biology (Barker, 1998). Previous work in the economics literature has also shown how pervasive conditions (e.g. malnutrition, sickness, pollution, etc.) in-utero and during the first years of life have considerable long-term consequences. Some of these studies identify effects of early life conditions on outcomes at adulthood, such as income, health, educational attainment, and physical and mental disabilities (Alderman et al., 2006; Almond, 2006; Almond and Mazumder, 2011; Maccini and Yang, 2009).

This paper investigates medium-term consequences of negative conditions experienced during early stages of life on children's physical and cognitive development. Test scores for language development, working and long-term memory, and visual-spatial thinking provide information about specific dimensions of cognitive development. This information, added to objective anthropometric measures (like height and weight) and gross motor skills, has been proven as a strong predictor of success later in adulthood (Case and Paxson, 2008; Grantham-McGregor et al., 2007). Therefore, identifying medium-term impacts of early-life conditions on these indicators provides

[2] Joint with Marta Vicarelli, Yale University

valuable information about the channels that might be driving previously identified long-term impacts.

Weather events have been widely used in the economics literature as instruments. Some examples include hurricanes, droughts, and rainfall events. To identify negative early-life conditions, this paper employs extreme precipitation shocks[3] that occurred during the 1998-1999 maize harvest seasons and were related to the "El Niño Southern Oscillation" (ENSO) climatic event. The occurrence of these shocks severely compromised crop outputs (SAGARPA, 2008). Using geographical variation in precipitation, we compare health, anthropometric and cognitive development outcomes of children exposed at early stages of life to the shock versus same-aged children not exposed. The population of children under analysis spans different stages of early child development: from *in-utero* conditions up to their second year of life. The main identification assumption is that the occurrence of these shocks is exogenous and creates negative conditions that potentially affect children at early stages of life (in-utero and first years after birth).[4]

The study of these shocks is interesting given ENSO's characteristics. ENSO is a recurrent climatic event with a 5 to 7 year cycle. It develops in the Pacific Ocean and affects global hydro-meteorological patterns, causing extreme weather events (e.g. droughts, floods, heat waves) with negative impacts on weather-sensitive industries, such as fishing and agriculture (Neelin et al., 1998).[5] Climatologists indicate that ENSO cycles will continue to affect global climate, and events might become more frequent and intense with global warming (Vecchi and Wittemberg, 2010). ENSO-related studies are therefore relevant from an economic, climatic, and public policy perspective. To the authors' knowledge this is the first study to investigate the impact of ENSO-related weather shocks on human capital formation.

The data used in this study comes from a rich longitudinal household dataset gathered as part

[3] The terms "extreme precipitation shocks" and "floods" are used interchangeably throughout the paper. Further details of the shocks identification are provided in *Section 1.3.2*.

[4] Some negative consequences of the shocks include: compromising the household's (expected) income flow, thus affecting food consumption and nutrition, and creating an unhealthy and stressful environment, among others.

[5] Further details about ENSO can be found in *Section 1.2*.

of Mexico's *Progresa* randomized poverty alleviation program.[6] The *Progresa* database is exceptional for size and data quality and includes biannual surveys from 1997 to 2000, as well as a detailed follow-up survey in 2003. This latter survey provides valuable information for children aged 2 to 6, namely, specific indicators of cognitive development, motor skills, as well as objective anthropometric and health indicators. Tests of high internal reliability and validity according to U.S. standards were used to provide cognitive development indicators: (i) the *Peabody Picture Vocabulary Test* was used to assess language development; and (ii) three sub-tests of the *Woodcock-Muñoz Test*[7] provided working and long-term memory, and visual-spatial thinking indicators (Schrank et al., 2005). Anthropometric and health variables include height, weight, hemoglobin, and self-reported health. Gross motor skill measures were obtained by administering the *McCarthy Scale of Children's Abilities Test*, and include balance and physical coordination.

To identify children exposed to ENSO-related weather shocks during their early stages of life, the *Progresa* database was spatially merged with a monthly precipitation gridded dataset using the child's household geographical location. The climatic data used is publicly available from the University of East Anglia Climate Research Unit, (UEA CRU-TS2p1) and includes interpolated monthly time-series from 1961 to 1999, with a spatial resolution of 0.5 x 0.5 degrees (Mitchell, 2005). The magnitude of the deviation from the historical average monthly rainfall level in a given grid is used to identify extreme precipitation events.[8]

The main findings in this paper indicate medium-term negative effects of excessive rain shocks on cognitive and anthropometric indicators. Children exposed to the shock during the first two years of their life suffered the most severe consequences. Language development, working memory, and visual-spatial thinking test scores of these children are 21, 19, and 13 percent lower than same-aged children not exposed, respectively. Also, they exhibit lower weight (0.84 lb.), height (0.71 in.), and higher likelihood of stunting (13 percentage points). Similarly, children born the

[6] *Progresa* changed its name to *Oportunidades* in 2002 and up to date is Mexico's most comprehensive social program in operation.

[7] Spanish version of the Woodcock-Johnson Tests of Cognitive Abilities.

[8] This is a standard practice recommended by climatologists (Heim, 2002; Keyantash and Dracup, 2004).

same year and up to one year after the shock obtain lower cognitive results (that range from 11 to 16 percent), lower height (0.49 in.), and higher likelihood of stunting (14 percentage points). No strong evidence of negative effects is found for gross motor skills.

Furthermore, the longitudinal structure of the dataset allows investigating which household's characteristics were most affected by the shock after its occurrence, and thus contributed to the negative medium-term consequences found in children. Our estimates show that the extreme rainfall events at the end of the harvest season represented an important negative income shock. Total household income, reported two months after the shock occurred, was 39 percent lower for households living in regions exposed. This negative income effect persisted up to two years after the shock occurrence. The value of food consumption (per adult equivalents) was 10 to 15 percent lower when comparing households in exposed versus non-exposed regions. Diet composition also had significant effects: up to two years after the shock, households in affected regions significantly reduced their animal-origin protein consumption, as well as fruits and vegetables. Finally, mother's self-reported measures about their children's sickness did not show any short nor medium-term effect from the shocks.

The final part of this paper tests whether *Progresa*, a conditional cash transfer program targeting poor rural households, helped mitigating the negative effects of ENSO-related rainfall shocks. *Progresa's* randomized evaluation phase took place between 1997 and 2000, which coincides with the ENSO event analyzed in this paper. This regional and temporal coincidence provides a great opportunity to assess the possible benefits of *Progresa* as an insurance mechanism against rainfall shocks.

Two empirical strategies were used for the *Progresa* analysis. First, the randomization at the village level is employed.[9] Given that the outcomes analyzed come from the 2003 follow-up survey, the comparison should be interpreted as an early versus late random allocation (rather than treatment versus control). Second, a regression discontinuity design is estimated using the ad-

[9] Villages that were selected for treatment began receiving the benefits in May 1998 while control villages were added between November and December 1999.

ministrative rule to select beneficiaries. This analysis is able to identify effects of being a program beneficiary from its start (1998) with respect to mid-2001.

No evidence of direct nor mitigating effects of *Progresa* on anthropometric and cognitive outcomes is found. Despite providing cash transfers that household's could choose how to spend, *Progresa* does not offset the negative effects on consumption and diet composition in the periods that follow the negative shock. Similarly, Paxson and Schady (2010) and Fernald and Gertler (2005) find slightly positive to no direct effects on anthropometric and cognitive development indicators from randomized poverty alleviation programs in Ecuador (*Bono de Desarrollo Humano*) and Mexico (*Progresa*), respectively.

The remainder of the paper is organized as follows. *Section 1.2* gives some background on ENSO and maize agriculture. *Section 1.3* describes the socioeconomic, child development and climatic datasets used. *Section 1.4* explains the identification strategy followed. *Section 1.5* details the results of the anthropometric, cognitive, and motor skills outcomes. *Section 1.6* analyzes the possible mechanisms that might be driving these medium-term outcomes. *Section 1.7* provides evidence from the *Progresa* analysis. Finally, *section 1.8* concludes.

1.2 Background on ENSO and its Effects

1.2.1 El Niño Southern Oscillation (ENSO)

ENSO is a recurrent quasi-periodic climatic event with a 5 to 7 year cycle and global meteorological impacts. It develops across the Pacific Ocean and combines two phenomena: (i) a positive sea-surface temperature anomaly in the eastern tropical Pacific called *El Niño*[10] (or *La Niña* in case of a negative temperature anomaly); and (ii) an atmospheric pressure anomaly in the western tropical Pacific Ocean (i.e the *Southern Oscillation*). ENSO oscillates between its two extremes: *El Niño* (warm event) and *La Niña* (cold event). Each phase typically lasts one year, with a peak in December, and then tapers down towards a neutral state.

[10] The term *El Niño* is the Spanish expression for *The Child*. It is a religious allegory that refers to the arrival of Child Jesus (or the *Nativity*) because the periodic warming of eastern Pacific, along the coasts of Peru and Ecuador was originally noticed after mid-December, around Christmas.

ENSO affects hydro-meteorological patterns around the world, causing extreme weather events such as droughts, floods, and heat waves (Ropelewski and Halpert, 1987; Philander, 1990; Neelin et al., 1998; Larkin and Harrison, 2005). Its strongest impacts are observed in countries bordering the Pacific Ocean, from Latin America to Southeast-Asia; however, ENSO's consequences reach regions as far as India and Africa (Cane et al., 1994).

ENSO-related changes in weather patterns influence the frequency and intensity of tropical storms, including a decrease (increase) in Atlantic hurricane activity (Gray, 1984) and an eastward (westward) shift of western Pacific cyclone activity during *El Niño* (*La Niña*) (Revell and Goulter, 1986; Chan, 2000). Changes in climatic patterns and oceanic circulation during ENSO events strongly influence terrestrial and marine ecosystems, and societies around the globe. *El Niño* and *La Niña* events tend to differ for onset, magnitude, spatial extent, duration and cessation (Ropelewski and Halpert, 1987; Philander, 1990; Allan, 2000). *Figure A.1* in the *Appendix A* shows the spatial distribution of regional precipitation anomalies, associated to different *La Niña* events occurred in late summer (September-October). This study will focus on the late-summer rainfall shocks related to the 1998-1999 *La Niña* event.

There is evidence suggesting that ENSO cycles have occurred for more than 6,000 years (Markgraf and Diaz, 2000), and will continue to occur and influence global climate in the future. Moreover, ENSO events might become more frequent and more intense; ENSO activity and characteristics appear to be strongly related to the tropical Pacific climate system, which is expected to change during the 21st century in response to climate change (Vecchi and Wittemberg, 2010). It is, therefore, of great interest to understand the nature and magnitude of ENSO impacts on society.

1.2.2 ENSO, weather and agriculture

ENSO periodically causes severe socioeconomic consequences in both developed and developing countries. The estimated costs of the two largest *El Niño* events of the twentieth century were: 8 to 18 billion U.S. dollars (USD) for the 1982-83 event (Wallace and Vogel, 1994; Sponberg, 1999), and 35 to 45 billion USD for the 1997-98 event (Sponberg, 1999). In developing countries, weak or absent insurance and credit markets make households employed in weather-sensitive industries (e.g. agriculture and fishing) particularly vulnerable to climatic events of this nature.

For this study, data was collected from Mexican poor rural areas where most of the households depend directly or indirectly on agriculture. Most of the farmers surveyed report growing maize under a rain-fed system (around 90% of the households). Maize represents the most important crop in Mexico. Between 1996-2006, maize production amounted for 51% of the surface planted, generated 7.4% of the total agricultural volume produced, and represented 30% of the value of total production. Maize has two main agricultural seasons: Spring-Summer (78.5% of total production) and Autumn-Winter (21.5%) (SAGARPA, 2008).

This study will focus on the Spring-Summer agricultural season, the most important in terms of production. The agricultural season includes three main stages: (i) planting (April-June), (ii) growing (July-August), and (iii) maturation and harvesting (September-November). Conde et al. (2004) indicate that April's rain is fundamental for a successful maize crop. If rain doesn't arrive by May, farmers usually switch their crop to other varieties that develop faster and have shorter cycles, mainly oat, which can be planted up to June.[11] Later, the growing season is vulnerable to lack of rain (Smith, 1995). Finally, the harvest season, which is the one we focus on in this study, is sensitive to hurricanes and flooding events (SAGARPA, 2008).

Figure 1.1 shows the rainfall distribution[12] in the area under study for the Spring-Summer agricultural seasons related to the 1997-1998 *El Niño* and 1998-1999 *La Niña* events. We choose to analyze the extreme rainfall events at the end of the 1999 agricultural season because of the high degree of spatial rainfall variability at the harvest season. As seen in *Figure 1.1*, the 1997-1998 *El Niño* was also characterized by droughts at the beginning of the agricultural season. The low

[11] A popular Mexican farmer's rhyme describes this behavior: "What Saint John doesn't see born (June 24th), Saint Peter considers lost (June 29th)" (authors' translation to the original: "Lo que San Juan no ve nacido, San Pedro lo da por perdido").

[12] The region under study is divided by 0.5 degree x 0.5 degree grids. The graph illustrates the distribution of rainfall standardized deviations from the 1961-1999 historic averages for the different grids.

variability of rainfall meant that most of the region under study was similarly affected by this shock. Households could react to droughts at the beginning of the agricultural season by shifting resources to other income generating activities, for example, migrating seasonally or permanently (Munshi, 2003). On the other end, extreme rainfall shocks at the end of the agricultural season were closer to negative income shocks given that all the investment of labor and resources had already been spent on the crop. Evidence from the households in the database used suggests that these rainfall shocks were unexpected.[13]

1.3 Data

1.3.1 Progresa data

The data used in this study is part of *Progresa's* randomized evaluation longitudinal database. It was collected biannually between 1997 and 2000 at 506 marginalized communities of rural Mexico. In 2003, a follow-up survey gathered specific information about children between 2 and 6 years old in a subset of the original villages (in addition to household socioeconomic data). The 2003 dataset includes information for 259 villages, 5,000 households, and 6,264 children on anthropometric, health, cognitive, and gross motor development indicators.[14]

Cognitive tests. The *Peabody Picture Vocabulary Test* (PPVT) and three subsections of the *Batería III Woodcock-Muñoz Test*[15] (WMT) are used as indicators of cognitive development. The PPVT measures the receptive vocabulary of children aged 3 to 6 by asking them to indicate which of four pictures best represents a stimulus word. Studies have found that vocabulary tests tend to be strong predictors of school success and contribute in a large extent on tests that assess general intelligence. The PPVT test is used in preschool aged children to assess early child development (Dunn and Dunn, 1986).

[13] Households do not report significant effects in change of land used or total area planted at the beginning of the agricultural season when comparing households in regions affected and not affected by the weather shocks used in the analysis.

[14] Data is publicly available at `http://evaluacion.oportunidades.gob.mx/evaluacion`

[15] The Spanish version of the Woodcock-Johnson test.

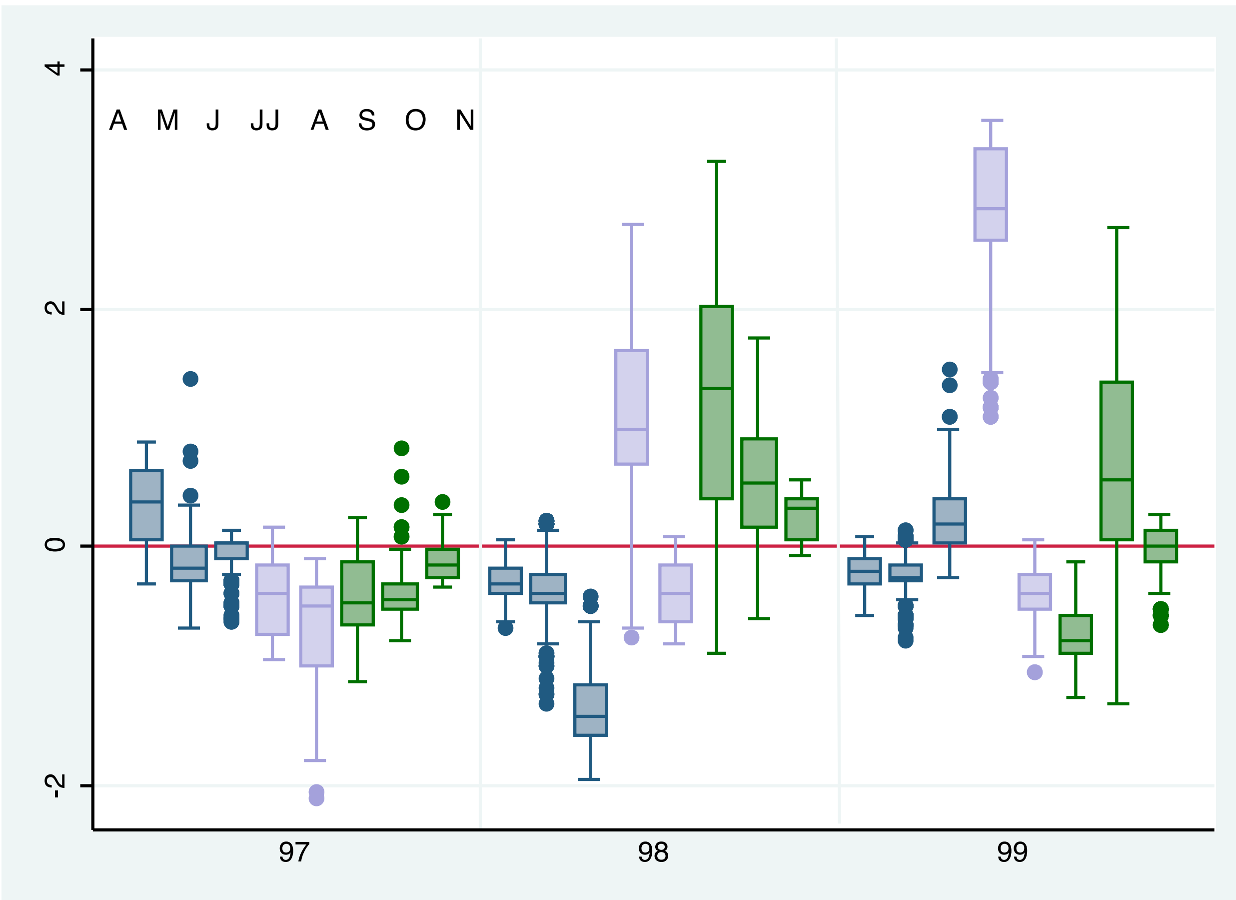

Figure 1.1: Distribution of Monthly Precipitation Standardized Anomalies.

This figure reproduces *Figure 1* in Vicarelli (2011). The x-axis represents the 1997, 1998 and 1999 agricultural seasons for maize. The agricultural season lasts eight months, from April to November, and includes the following phases : planting Phase (April-June); growing phase (July to August); and maturation and harvesting (September to November). The y-axis represent that average monthly precipitation standardized anomaly for the grid-cells where the *Progresa* villages are located. The unit of observation is a 0.5 x 0.5 degree grid-cell (Total=55 grid-cells). For each grid-cell, the monthly standardized deviation from the 1961-1999 mean is calculated.

Three subtests of the WMT were used to measure long-term memory, working memory, and visual-spatial thinking for children 2 to 6 years old. These abilities are measured, respectively, by requiring children to: learn associations between unfamiliar auditory and visual stimuli; remember and repeat single words, phrases, and sentences; and identify an object's picture from a partial drawing or representation. Schrank et al. (2005) describe these abilities as follows: (i) long-term memory is the ability to store information and fluently retrieve it later; (ii) working memory (also referred to as short-term memory) is the capacity to hold information in immediate awareness while performing a mental operation on the information; and (iii) and visual-spatial thinking is the ability to perceive, analyze, synthesize, and think with visual patterns, including the ability to store and retrieve visual associations. Because of their high *internal reliability* and *validity*,[16] the WMT and the PPVT are regularly selected to evaluate early childhood abilities and have been found to be good predictors of later school achievement (Duncan et al., 2007).

Anthropometric variables. The 2003 *Progresa* follow-up survey also includes objective measures of *height* and *weight* collected by a qualified nurse for all the children in the sample. The binary variable *stunting* is constructed based on the WHO definition: equal to *one* if the child's height is two or more standard deviations below the age-sex standardized height of a healthy reference population (World Health Organization, 2012). *Stunting*, or low weight for age, usually reflects insufficient nutrient intake during early stages of development. It generally occurs before age two and once established, it is usually permanent (most children never gain the height lost nor achieve a normal body weight). Consequences may be extremely severe: a stunted growth may lead to premature death later in life due to incomplete development of vital organs during childhood. Less extreme effects also include delayed development, impaired cognitive function, and poor school performance (UNICEF, 2007).

[16] In educational testing, *internal reliability* indicates the degree to which test scores for a group of test takers are consistent over repeated applications of the measurement procedure. *Validity* refers to the degree to which accumulated evidence and theory support specific interpretations of the test scores (American Educational Research Association et al., 1999).

Health indicators. Blood samples were also gathered for all children as part of the 2003 data collection. By using hemoglobin levels, adjusted for village altitude, an indicator for *anemia* is generated based on the World Health Organization standards (Ruiz-Argüelles and Llorente-Peters, 1981). *Anemia* is usually an indicator of poor nutrition (mainly iron deficiency) and poor health. Its negative consequences range from lower cognitive and physical development to increased risk of mortality (World Health Organization, 2008). An additional measure of child's health comes from mothers' survey responses. Mothers were asked to report the number of days that their children were sick during the previous month and unable to perform their regular activities. This number corresponds to the variable *days_sick*.

Gross motor skills. Gross motor skills are central to the successful performance of school tasks and were evaluated using a section of the *McCarthy Scale of Children's Abilities* (MSCA) (McCarthy, 1972). Besides school failure, difficulty or inability to perform manual jobs can be debilitating for young adults in rural areas and have broad long-term socioeconomic consequences. Deficiencies in gross motor coordination (e.g. poor balance, poor timing and coordination, difficulty combining movements into controlled sequences) may also reflect neuromotor and executive-function deficits (Poltajko et al., 1995).

The MSCA tests, administered to all children between 2 and 6, focused on leg coordination: the first tests required children to stand on one foot and measured both the ability to perform the task and the amount of time endured staying in balance (in seconds); the second and third tests assessed the ability to walk backwards and to walk straight following a line, respectively.

Table 1.1 provides descriptive statistics for all the outcomes as well as controls that will be used in the empirical specification.

Table 1.1: Descriptive statistics

Variable	Num. obs	Mean	Std. Dev.	Min	Max
Anthropometrics and health					
weight (lb)	6233	33.33	6.6580	16.31	122.14
height (in)	6209	37.93	3.5430	16.81	56.54
stunting (binary)	4684	0.38	0.4842	0	1
anemia (binary)	6215	0.27	0.4415	0	1
sick days	5598	1.39	2.7874	0	30
Cognitive tests					
language (Peabody)	4671	2.25	0.8946	0	4.37
LT memory (WM 1)	6010	2.17	0.8818	0	4.17
ST memory (WM 2)	5747	2.95	0.6293	0	4.01
visual-spatial (WM 3)	4988	2.30	0.5959	0	3.74
Motor skills					
balance (seconds)	5943	8.29	5.0580	0	45
walk back (binary)	6088	0.89	0.3180	0	1
walk str (binary)	6041	0.83	0.3735	0	1
Controls					
age (months)	8173	50.26	16.44	19	82
male (binary)	8173	0.51	0.50	0	1
HH Poverty index (1997)	5254	703.06	134.67	237	1239
HH size (1997)	5268	6.08	2.56	1	24
HH head language					
* spanish & indigenous	5281	0.4	0.49	0	1
* only indigenous	5281	0.04	0.18	0	1
Distribution of birth cohorts					
coh97	8173	0.17	0.3793	0	1
coh98	8173	0.21	0.4099	0	1
coh99	8173	0.22	0.4128	0	1
coh00	8173	0.21	0.4085	0	1
coh01	8173	0.18	0.3863	0	1

Continued on next page

Table 1.1 – continued

Variable	Num. obs	Mean	Std. Dev.	Min	Max
Shocks by birth cohort					
coh97*rain_shock	8173	0.11	0.3110	0	1
coh98*rain_shock	8173	0.13	0.3351	0	1
coh99*rain_shock	8173	0.13	0.3388	0	1
coh00*rain_shock	8173	0.13	0.3341	0	1
coh01*rain_shock	8173	0.11	0.3186	0	1

Sample restricted to children aged 2-6 in 2003.

1.3.2 Climatic data

Monthly precipitation data available from the University of East Anglia Climate Research Unit (UEA CRU -TS2p1) is used to measure the presence of rainfall shocks in the region under analysis. The monthly series are available as interpolated gridded data with a spatial resolution of 0.5 x 0.5 degrees (Mitchell, 2005). This dataset is spatially merged with the *Progresa* dataset using the geographical location of the village where each child was born. The 259 *Progresa* villages are distributed over 55 grids. The number of villages per grid varies, from a minimum of 1 to a maximum of 20.

In the estimations, a binary variable for the ENSO-related rainfall shock (*rain_shock*) is used to analyze the impact of negative conditions during early stages of life on children's outcomes. The variable was constructed using each grid's *standardized precipitation anomaly*. The *standardized precipitation anomaly* indicates the number of standard deviations from the long-term mean (1961-1999) for each grid-month pair. A rainfall shock is identified ($rain\_shock = 1$) whenever the *standardized precipitation anomaly* is above 0.7 standard deviations in September or October of 1999 (harvest months). The threshold to identify the weather shocks comes from conversation with climatologists who indicated that this level is already dangerous (destructive) for the crop during the harvest season. Nonetheless, in *section 1.5.6*, a sensitivity analysis will consider changing the 0.7 standard deviations cutoff to 0.5 and 1 to assess the relevance of the cutoff point used to define the shock. *Figure 1.1* shows the monthly distribution of the standardized precipitation anomalies used to define the rainfall shocks.

The use of the *standard precipitation anomalies* to identify the shocks is supported by extensive applications in the climatology literature (Heim, 2002; Keyantash and Dracup, 2004). The decision to use a binary variable for the rain shocks was motivated by two main reasons: (i) the qualitative evidence found on weather reports indicates substantive loss of crops as a result of floods, therefore, the relation between crop output and rainfall would not be easily fitted with a parametric functional form; and (ii) the use of the binary variable aids the ease of interpretation of the results. Furthermore, the use of different thresholds in the robustness checks informs about the pattern of the results with respect to the *standardized precipitation anomalies*.

1.4 Empirical Specification

The following specification seeks to identify the medium-term effect of excessive rainfall shocks occurring at early stages of children's development on anthropometric, health, cognitive development and gross motor skill outcomes. The analysis considers children born between 1997 and 2001.

$$Y_{ij} = \left(\sum_{k=1997}^{2001} \gamma_k coh\_k_{ij} + \eta_k rain\_shock_j * coh\_k_{ij} \right) + \beta X_{ij} + \nu_j + \epsilon_{ij} \tag{1.1}$$

where Y_{ij} is the outcome for individual i in pixel j , coh_k_{ij} is an indicator for individual i in pixel j of being born on year (cohort) k, $rain_j$ is an indicator for a weather shock occurrence in pixel j, X_{ij} are controls for individual i in pixel j, and ν_j gives pixel-clustered standard errors.

Y_{ij} refers to the set of outputs under analysis that include: (i) anthropometric variables, such as weight, height, and stunting; (ii) the logarithm of cognitive test results, which include the Peabody test and three subsections of the Woodcock-Muñoz test; (iii) health indicators, such as *anemia* and self-reported health; and (iv) motor skills coordination results, which include variables from the McCarthy test.

On each estimation, the main parameter of interest will be η_k. Given that the rain shock used for the estimations took place in a specific year (1999), the η_k parameter will indicate the effect of the shock for children in a given development stage with respect to same-aged children that were not affected by the shock. For example, η_{1997} will give the effect of the shock on children that were one to two years old at the time of the shock with respect to same-aged children not affected.

Exogeneity Test. The main identification assumption is that the occurrence of the shocks is exogenous and generated negative conditions that affected children at early stages of life. To test the exogeneity assumption of the shocks, the longitudinal feature of the dataset is employed. Using the baseline data from 1997, which corresponds to household's information before the rainfall shock took place, a group of indicators is aggregated at the village level. *Table 1.2* shows the results from testing the difference of means for several observable indicators extracted from the household's survey.[17] The statistics show that for most observable characteristics there is no difference between villages affected and those not affected by the rainfall shocks at the baseline.

Table 1.2: Exogeneity tests for excessive rainfall shocks. Columns (1) and (2) present the mean values of each variable for villages not-exposed to rainfall shocks ($rain\_shock_j = 0$) and exposed to rainfall shocks ($rain\_shock_j = 1$), respectively. Column (3) and (4) report the difference of the two means and the corresponding t-statistics.

	Mean $rain\_shock_j = 0$ (1)	Mean $rain\_shock_j = 1$ (2)	Difference (3)	t-statistic (4)
Village characteristics				
male avg. wages	318.3	303.2	15.17	1.248
female avg. wages	41.81	45.44	-3.629	-0.980
Household characteristics and assets				
size	6.748	6.827	-0.0792	-0.683
Poverty index	712.7	705.5	7.189	0.670
owns land (ha)	1.749	1.727	0.0215	0.128
own house (binary)	0.940	0.936	0.00387	0.338
electricity (binary)	0.776	0.761	0.0154	0.373
water (binary)	0.0395	0.0448	-0.00533	-0.567

Continued on next page

[17] The tests were also done using individual level data, and comparing individuals living in localities exposed to the shock with those living in localities not exposed. The same results are derived whether the test uses individual or grid level data.

Table 1.2 – continued				
	Mean $rain\_shock_j = 0$	Mean $rain\_shock_j = 1$	Difference	t-statistic
tv (binary)	0.617	0.471	0.147***	4.360
vehicle (binary)	0.136	0.0599	0.0764***	4.615
donkeys	0.421	0.384	0.0371	0.691
bullocks	0.130	0.129	0.000910	0.0158
sheep	1.695	1.606	0.0888	0.234
chickens	6.719	7.933	-1.213**	-2.666
pigs	1.151	1.322	-0.171	-1.160
Household migratory characteristics				
temporary migrants	0.0463	0.0392	0.00715	1.407
permanent migrants to:				
US	0.0392	0.0119	0.0274*	2.548
Mexico	0.0236	0.0221	0.00151	0.186
Head of household characteristics				
male (binary)	0.904	0.889	0.0147	1.199
age (years)	43.11	41.79	1.319	1.965
education (years)	3.759	3.597	0.162	1.028
agric worker	0.701	0.738	-0.0373	-1.386
language spoken:				
Indigenous	0.00681	0.0357	-0.0289***	-3.469
Spanish & Indigen.	0.177	0.397	-0.220***	-4.704

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Spatially-correlated standard errors. The estimation of equation 1.1 adjusts for clustered standard errors by grid. This assumption allows for correlation between observations geographically located in the same grid-cell (i.e. pixel). However, it also assumes that errors of observations located in adjacent grids are independent from each other. In the economic literature, a growing number of studies that use geographical data have adopted an alternative solution that allows for correlation between observations closely located (Dell et al., 2009; Deschênes and Greenstone, 2006). The strategy is based on Conley (1999) work, who proposed a methodology to correct for spatial correlation when estimating the standard errors. Conley's correction consists in allowing the variance-covariance matrix to have correlated standard errors if the observations are located within a pre-specified distance threshold (the threshold has to be assumed).[18] The main estimates in this paper include clustered and spatially correlated standard errors (with 1 decimal degree cutoff assumed). A sensitivity analysis for the standard errors is included in the supplementary material.

1.5 Results

1.5.1 Effects on anthropometric and health indicators

Table 1.3 presents the estimated effects of excessive rainfall shocks during the 1999 harvest season on children's anthropometric indicators (*height*, *weight*, and a binary indicator for *stunting*), and health outcomes (a binary indicator for *anemia*, and self-reported number of sick days during the previous month, *days_sick*).[19]

Results show significant lower weight and height for children that were exposed during the first two years of life to the shock (0.84 lb. lower weight and 0.71 in. lower height for those born in 1998, and 0.47 in. lower height for those born in 1997) with respect to same-aged children not exposed. Similarly, children born the same year and one year after the shock occurred, exhibit negative effects on height with respect to same-aged children not affected (0.56 in. and 0.43 in., respectively).

The negative impacts on height are substantive enough to significantly increase the probability of stunting. Children born between 1998 and 2001 are significantly more likely to be stunted if they were exposed to the 1999 rainfall shock. The probability of stunting is 14 percentage points higher for children born in 1999 and 2000, and 12 percentage points higher for children born in 1998 and 2001 compared to same-aged children not affected by the shock).

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[18] See Conley (1999) for further details about this methodology. Statistical codes to correct for spatial correlation are also available on Timothy Conley's website.

[19] Similar results were found using as main independent variable the indicator for excessive rainfall shocks occurred in 1998 and can be made available upon request.

There is no evidence to suggest that anemia and children's number of sick days (reported by their mothers) are significantly affected by the weather shocks.

Table 1.3: Effect of the 1999 September-October rainfall shock on anthropometric indicators measured in 2003 for children born between 1997 and 2001.

	weight (lb)[a]	height (in)[a]	stunting[b]	anemia[c]	days_sick[d]
	(1)	(2)	(3)	(4)	(5)
coh97 x rain_shock[e]	-0.613	-0.466**	.	-0.0323	0.346
	[0.6137]	[0.2190]	.	[0.0624]	[0.2652]
	(0.6565)	(0.2629)		(0.6)	(0.2421)
coh98 x rain_shock	-0.837*	-0.709***	0.127*	-0.0132	0.00235
	[0.4806]	[0.2176]	[0.0722]	[0.0336]	[0.1813]
	(0.5352)	(0.2475)	(0.0712)	(0.0394)	(0.1781)
coh99 x rain_shock	-0.733	-0.555**	0.140**	0.00325	-0.232
	[0.4399]	[0.2483]	[0.0672]	[0.0368]	[0.1860]
	(0.4924)	(0.2826)	(0.0720)	(0.0364)	(0.2170)
coh00 x rain_shock	-0.145	-0.426*	0.140**	0.00877	-0.00481
	[0.3484]	[0.2485]	[0.0630]	[0.0338]	[0.2043]
	(0.3378)	(0.2514)	(0.0658)	(0.0368)	(0.2007)
coh01 x rain_shock	-0.468	-0.189	0.115*	0.00671	0.0899
	[0.3742]	[0.2052]	[0.0653]	[0.0616]	[0.2517]
	(0.3991)	(0.1867)	(0.073)	(0.0716)	(0.2211)
Observations	3729	3705	2777	3765	3377
R^2	0.58	0.76	0.09	0.03	0.02
Mean	33.42	38.08	0.384	0.273	1.282

Controlling for age (months), age^2, gender, father's language, HH structure, cohorts, poverty index (1997).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered by grid in brakets; Conley standard errors in parentheses (cutoff= 1 degree).

[a] Weight and height are measures in pounds and inches, respectively.

[b] Stunting is a binary variable = 1 if the child is stunted. Stunting is defined as being two or more standard deviations below the age-sex standardized height of a healthy reference population (World Health Organization, 1996).

[c] Anemia is a binary variable = 1 if the child is anemic. Anemia is defined as hemoglobin less than 11 g/dL adjusted for altitude using standard adjustments (Ruiz-Argüelles and Llorente-Peters, 1981).

[d] Number of days in the previous 4 weeks that the child was reported sick by the mother.

[e] *coh97 x rain_shock* indicates the interaction between the variable *coh97* (=1 if the child was born in 1997) and the variable *rain_shock* (=1 if a rainfall shock occurred in 1999).

1.5.2 Effects on cognitive development

As described on *Section 1.3.1*, the 2003 *Progresa* survey includes several specific cognitive tests: language skills (Peabody Test), long-term memory, short-term memory, and visual-spatial thinking (Woodcock-Muñoz Test). For the estimations, we use as outcomes the logarithm of the test scores.

Table 1.4 reports the estimated effects of excessive rainfall shocks during the 1999 harvest season on the test scores that measure specific cognitive abilities . The estimates suggest negative and significant effects of the rainfall shocks on language development, long-term memory, and visual-spatial thinking abilities. The larger negative effects are mostly found on children who were affected by the shock during their first or second year of life (i.e. children born in 1997 and 1998). This group of children exhibits 21, 19, and 13 percent lower test scores on the language, long-term, and visual-spatial thinking tests, respectively, with respect to same aged-children not affected by the shock. Lower (in absolute terms), but still significant negative effects are found for children born the year of the shock (1999) or the following year (2000); these negative outcomes range, in absolute value, from 11 to 14 percent. No significant effects were found in short-term memory test scores.

1.5.3 Effects on gross motor skills

Table 1.5 shows the effects on gross motor skills measured with outcomes from the *McCarthy Scale of Children's Abilities Tests*. No strong and consistent evidence of negative effects of the shocks is found for these outcomes. Balance is the only outcome for which minor negative effects from the rainfall shocks are found, being the effect on the 1997 cohort the only statistically significant (decrease in 0.7 seconds in the ability to hold balance with one foot).

1.5.4 Correction for spatially-correlated standard errors

Tables 1.3, 1.4, and *1.5* show the main estimation results calculated with clustered SEs by grid (shown in brackets) and, alternatively, using Conley's proposed corrections for spatial correlation with a 1 decimal degree cutoff (shown in parentheses). As evidenced in the tables, some of the standard errors increase as a result of allowing for spatial correlation, but this increase tends to be small and does not affect the statistical significance of the results. *Table A.1* summarizes three alternatives for the calculation of the standard errors: clustered by pixels, and two estimates of Conley SEs using 1 and 2 decimal degrees thresholds, respectively. The standard errors for the anthropometric estimations are the most sensitive to spatial correlation correction, but overall, the significance of the results remains.

Table 1.4: Effect of the 1999 September-October rainfall shock on cognitive development indicators measured in 2003 for children born between 1997 and 2001. (Outcomes are the logarithm of test scores).

	Peabody Test[a]	**Woodcock-Muñoz Test**[b]		
	language	long term memory	short term memory	visual-spatial thinking
	(1)	(2)	(3)	(4)
coh97 x rain_shock[c]	-0.216**	-0.194*	-0.000168	-0.133***
	[0.0967]	[0.1042]	[0.0257]	[0.0424]
	(0.1055)	(0.1199)	(0.0288)	(0.0496)
coh98 x rain_shock	-0.209***	-0.199***	0.0139	-0.121***
	[0.0601]	[0.0644]	[0.0322]	[0.0335]
	(0.0679)	(0.0817)	(0.0363)	(0.0398)
coh99 x rain_shock	-0.148**	-0.162***	-0.0532	-0.111**
	[0.0682]	[0.0598]	[0.0370]	[0.0493]
	(0.0651)	(0.0649)	(0.0357)	(0.058)
coh00 x rain_shock	-0.0258	-0.147**	0.0173	-0.143**
	[0.0672]	[0.0568]	[0.0374]	[0.0541]
	(0.0576)	(0.0565)	(0.0356)	(0.0486)
coh01 x rain_shock	-0.225	-0.0300	-0.0755	0.0364
	[0.6933]	[0.0549]	[0.0552]	[0.0899]
	(0.6910)	(0.66)	(0.0493)	(0.1003)
Observations	2835	3522	3385	2967
R^2	0.33	0.32	0.48	0.38

Controlling for age (months), age^2, gender, father's language, HH structure, cohorts, poverty index (1997).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors clustered by grid in brakets; Conley standard errors in parentheses (cutoff= 1 degree).

[a] Peabody Test measures language development. Peabody test scores are a reliable predictor of achievements in primary school.

[b] Woodcock-Muñoz Test is used to assess a wide range of cognitive abilities: long-term memory, short-term memory and visual spatial thinking.

[c] *coh97 x rain_shock* indicates the interaction between the variable *coh97* (=1 if the child was born in 1997) and the variable *rain_shock* (=1 if a rainfall shock occurred in 1999).

Table 1.5: Effect of the 1999 September-October rainfall shock on gross motor skills measured in 2003 for children born between 1997 and 2001. These gross motor skills are central to the successful performance of school tasks.

	balance (seconds)[a] (1)	ability to walk backward[b] (2)	ability to walk straight[b] (3)
coh97 x rain_shock[c]	-0.718**	0.0228	0.0140
	[0.3420]	[0.0162]	[0.0117]
	(0.3686)	(0.0186)	(0.0109)
coh98 x rain_shock	-0.164	0.0136	0.0220*
	[0.2609]	[0.0130]	[0.0118]
	(0.2628)	(0.0119)	(0.0104)
coh99 x rain_shock	-0.357	0.0174	-0.0371**
	[0.3408]	[0.0155]	[0.0156]
	(0.3452)	(0.0152)	(0.0134)
coh00 x rain_shock	-0.210	-0.0216	-0.00701
	[0.3739]	[0.0283]	[0.0374]
	(0.2811)	(0.0246)	(0.0351)
coh01 x rain_shock	0.0831	-0.0308	-0.0514
	[0.4352]	[0.0563]	[0.0509]
	(0.3796)	(0502)	(0.0488)
Observations	3563	3693	3663
R^2	0.33	0.16	0.24
Mean	8.286	0.891	0.843

Controlling for age (months), age^2, gender, father's language, HH structure, cohorts, poverty index (1997).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Standard errors clustered by grid in brakets; Conley standard errors in parentheses (cutoff= 1 degree).

[a] The child's ability to keep her/his balance on one foot is measured in seconds.

[b] The binary variables = 1 if the child was able to successfully complete the task.

[c] *coh97 x rain_shock* indicates the interaction between the variable *coh97* (=1 if the child was born in 1997) and the variable *rain_shock* (=1 if a rainfall shock occurred in 1999).

1.5.5 Persistent effects of the shock

Exposure to weather shocks can have not only immediate impacts on children's life conditions, but also persistent effects over multiple years, such as an extended reduction of income and consumption. *Section 1.6* will investigate some of the mechanisms that might be contributing to the negative effects on children's cognitive and anthropometric indicators. That section will provide further evidence of the prolonged effects that the shocks had on these mechanisms.

Results for both anthropometric and cognitive outcomes indicate that children in their first or second year of life at the time of the shock were more severely affected. However, this does not necessarily mean that the shock had stronger effects if exposure occurred in early childhood rather than in-utero. Given the persistent effects of the shocks on some of the mechanisms, this could also mean that these children were exposed to the shock for a longer period of time. Similarly, estimates in *Tables 1.3* and *1.4*, suggesting that children born one year after the shock (in 2000) are negatively affected, could also result from negative conditions in-utero and shortly after birth.

1.5.6 Robustness checks

Adding controls from the exogeneity test. As discussed in *Section 1.4*, the empirical specification of this study is based on the assumption that the rain shock is exogenous. The evidence presented in *Table 1.2* supports this hypothesis. As a robustness test, the few variables that presented significant differences in means in *Table 1.2*[20] are added to the estimation. If the initial estimates presented are capturing regional effects, it is likely that these additional control variables will pick up some of that effect, thus altering the main coefficients. *Tables A.2, A.3,* and *A.4* show that the main estimates do not change significantly when these additional controls are added.

Sensitivity analysis for different weather shock cutoffs. A second robustness test consisted on varying the threshold value used to define the rain shock variable (*rain_shock*). For this sensitiv-

[20] The household characteristics presenting significantly different means are: (i) TV, (ii) vehicle, and (iii) poultry ownership, (iv) household with permanent migrants at the U.S., (v) an indicator for household head speaking an indigenous language, and (vi) an indicator for household head speaking both Spanish and an indigenous language.

ity test we adopted two additional cutoff points: 0.5 and 1 standard deviation. *Tables A.5, A.6* and *A.7* suggest that both thresholds – using 0.5 and 1 standard deviations to define the shock – yield similar results to the original estimates. Adopting the lower 0.5 threshold, produces larger significant coefficients; the reverse is observed when the more stringent cutoff of 1 standard deviation is employed. These results suggest that lower precipitation anomalies are enough to negatively affect children. When the 1 standard deviation cutoff is adopted, some children that were actually exposed to the shock, are erroneously included in the control group, yielding a lower estimate for the shock.

1.6 Mechanisms Driving the Results

This section explores some of the mechanisms that might be driving the medium-term effects of the rain shock on children's cognitive development and anthropometric outcomes. To do this, we exploit the longitudinal design of the database, spanning three years from 1998 to 2000, to analyze immediate and persistent effects of the weather shock on household dimensions that may affect the child's growing environment. We adopt the model described in equation 1.1 to estimate the effect of the rainfall shock on these possible intermediate mechanisms. As in the previous analysis, the underlying assumption for the identification is the exogeneity of the shock. *Tables 1.6* and *1.7* show the main results of the mechanisms analysis.

Income. In response to shocks, households may experience income reductions with possible consequent contractions in consumption. We estimate the effect of rainfall shocks on total household income and income from agricultural activities measured in the year of the shock (period t) and up to two years after the shock (at periods $t+1$ and $t+2$).

Evidence presented in *Table 1.6* indicates that households exposed to the shock have lower total income than those not affected, being the effect persistent over three periods t, $t+1$, $t+2$: from about 40% decline in period t to 26% in period $t+2$. Results for income from agricultural activities are comparable: from about 28% decline in period t to 18% in period $t+2$.

Government aid. Post-shock governmental food and non-food aid might help smooth consumption, particularly food consumption. The *Progresa* dataset allows us to assess if villages exposed to shocks are more likely to have benefited from government transfers. Evidence presented in *table 1.6* shows that the probability of receiving government food aid increases by 5% immediately after the shock for households living in affected villages. Nevertheless, the government aid did not seem to have neutralized the negative effect of the shocks on medium-term children's outcomes.

Informal transfers. In rural villages, informal insurance strategies aimed at smoothing post-shock consumption include food and non-food transfers from relatives or neighbors. From the data available it is possible to see if there was a response to the shocks in terms of family or neighbor related transfers. The results presented in *table 1.6* show no significant changes in the probability of receiving informal transfers from family members immediately after the shock. However, we do observe a significant decrease of 3% in the probability of receiving transfers from neighbors. Weakening of intra-village transfers may be explained by the fact that neighbors were also exposed to the rain shock.

Consumption and diet composition. The negative effect on income, paired with absence of formal insurance and credit markets, and the weakening of informal safety nets (e.g. intra-village transfers), may lead to consumption contractions. Non-food consumption is usually the first portion of household consumption to be reduced. When these reductions are insufficient to protect food consumption and savings are not available, households must inevitably reduce the value of their food consumption, often by adopting changes in their diet composition or even by reducing their food intake.

We estimate the effect of excessive rainfall shocks on food consumption and in diet composition at periods t, $t+1$ and $t+2$. Results in *Table 1.7* show contractions in the value of food consumption over the three periods (10% on t, 11% on $t+1$, and 15% on $t+2$) for households exposed to rainfall shocks with respect to those not exposed. These estimates confirm the results found by Vicarelli (2011) using data for all the *Progresa* villages (506) included in the pilot phase.

Table 1.6: Effect of rainfall shocks on income at different periods t, t+1, and t+2; as well as on the probability of receiving formal and informal transfers immediately after the shock. Formal transfers include food and other forms of government aid. Informal transfers include food and other transfers from either a family member or from a neighbor.

Dependent Variables	Binary Variable (✓)	Coefficient[a]	Std_Dev
Total household income (log)			
$income_t$		-0.395***	(0.052)
$income_{t+1}$		-0.291***	(0.052)
$income_{t+2}$		-0.263***	(0.042)
Household income from agriculture (log)			
$agricultural\_income_t$		-0.276***	(0.047)
$agricultural\_income_{t+1}$		-0.268***	(0.056)
$agricultural\_income_{t+2}$		-0.181***	(0.044)
=1 if household received government aid			
$food\_aid_t$	✓	0.047**	(0.021)
$other\_aid_t$	✓	0.006	(0.034)
=1 if household received informal transfers			
$from\_family_t$	✓	-0.042	(0.043)
$from\_neighbor_t$	✓	-0.028***	(0.009)

Standard errors clustered by gridcell [in brackets].

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

[a] Each line shows the result of separate regressions. Control variables for each model include: (i) household's head characteristics (age, male, years of education, language, sector of employment, marital status); (ii) household characteristics (size, structure, baseline poverty index in 1997); and (iii) village characteristics (average male and female wage).

A reduction in the monetary value of food consumption is likely to lead to a dietary shift towards cheaper foods. As expected, three main changes in diet composition are also found in households exposed to the shock, compared to households not exposed. First, consumption of tortillas[21] increased immediately after the shock (13%), but later decreased in period $t + 2$ (22%). Second, consumption of animal-origin products decreased in periods $t + 1$ and $t + 2$ by 14% and 17%, respectively. Lastly, consumption of fruit and vegetables decreased by 20% in period t and 14% in period $t + 2$. This shift towards cheaper foods, by privileging tortillas over the consumption of nutritious foods rich in animal proteins (e.g. meat, fish, eggs, milk), might have negative consequences on the health conditions and development of young children.

Health of household members and medicine expenditures. Worse health conditions for children in household exposed to weather shocks immediately after the shock might point to health as a possible channel for the medium-term results. *Table 1.7* presents estimates of the impact of the rainfall shock on two health-related measures: the proportion of children reported sick by the mother within each household (*children_sick*) and medicine expenditures (*medicine_expenditure*) immediately after the weather shock (period t) and up to two years after its occurrence. The results seem to suggest that: health conditions reported by the mother were not affected; and medicine expenditures decreased for households exposed to shocks by 36% in period t and 30% in period $t + 1$. Nonetheless, results for medicine expenditure are very likely to be driven by liquidity constraints rather than changes in health status.

[21] Maize tortillas are the main food staple in Mexico.

Table 1.7: Effect of rainfall shocks on food consumption, diet composition, child health, and medicine expenditures. All responses are estimated up to two years (t, $t+1$, $t+2$) after exposure to the shock.

Dependent Variables	Coefficient[a]	Std_Dev
Food consumption (log)		
$food\_consumption_t$ [pesos]	-0.100***	(0.035)
$food\_consumption_{t+1}$ [pesos]	-0.115***	(0.034)
$food\_consumption_{t+2}$ [pesos]	-0.149***	(0.055)
$food\_consumption_t$ [kg]	-0.027	(0.036)
$food\_consumption_{t+1}$ [kg]	0.003	(0.030)
$food\_consumption_{t+2}$ [kg]	0.042	(0.057)
Diet composition (log)		
$tortilla\_consumption_t$ [pesos]	0.132**	(0.054)
$tortilla\_consumption_{t+1}$ [pesos]	-0.085	(0.064)
$tortilla\_consumption_{t+2}$ [pesos]	-0.218***	(0.069)
$animal\_consumption_t$ [pesos]	-0.052	(0.085)
$animal\_consumption_{t+1}$ [pesos]	-0.145*	(0.075)
$animal\_consumption_{t+2}$ [pesos]	-0.171**	(0.076)
$fruit\_and\_vegetable\_consumption_t$ [pesos]	-0.200***	(0.057)
$fruit\_and\_vegetable\_consumption_{t+1}$ [pesos]	-0.078	(0.049)
$fruit\_and\_vegetable\_consumption_{t+2}$ [pesos]	-0.143**	(0.056)
Children reported sick by the mother		
$children\_sick_t$ (% in the HH)	0.011	(0.022)
$children\_sick_{t+1}$ (% in the HH)	0.025	(0.027)
$children\_sick_{t+2}$ (% in the HH)	0.016	(0.031)
Medicine Expenditure (log)		
$medicine\_expenditures_t$	-0.361***	(0.121)
$medicine\_expenditures_{t+1}$	-0.303**	(0.113)
$medicine\_expenditures_{t+2}$	0.043	(0.128)

Standard errors clustered by gridcell [in brackets]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[a] Each line shows the result of separate regressions. Control variables for each model include: (i) household's head characteristics (age, male, years of education, language, sector of employment, marital status); (ii) household characteristics (size, structure, baseline poverty index in 1997); and (iii) village characteristics (average male and female wage).

1.7 The Role of *Progresa*

In 1997, villages in the region under analysis were selected to be included in a governmental conditional cash transfer (CCT) program, called *Progresa*. This section describes the program and investigates its potential mitigating effects against the negative consequences of exposure to the rainfall shock for households eligible to receive the program.

1.7.1 Brief *Progresa's* background

Progresa, now called *Oportunidades*, is a conditional cash transfer program initiated in 1997 by the Mexican government. Nowadays, it is Mexico's most comprehensive and extensive poverty alleviation program with a 5.6 million households' coverage (Mexican Ministry of Development, 2011). Its purpose is to break the intergenerational cycle of poverty through a combination of health and education interventions. The delivery of the cash transfers is conditional on children's school attendance, as well as periodic health check-ups of all family members. The amounts of the transfers vary mainly by the number, age, and gender of the children at school age. Up to date, households receive on average US$588 per year, which corresponds to 0.6 times the amount an individual would earn working for a minimum salary and 0.36 times what the fifth decile household earns.

By design, the intervention included a randomized program evaluation that took place between 1997 and 2000, with follow-up surveys in 2003 and 2007 to assess its short and medium-term benefits (Skoufias, 2001; Behrman et al., 2005). The identification strategy of eligible households occurred in three stages. First, 506 poor rural communities from seven different states were selected for the sample. These localities were identified in the 1990 and 1995 Censuses as highly marginalized rural communities[22] with at least fifty households and access to both education and health services. Second, within each community, a baseline socioeconomic survey ENCASEH (Encuesta de Características Socioeconómicas de los Hogares) was administered to all households on November 1997. This information was used to construct a *poverty index* for each household based on its asset ownership and socio-economic characteristics of its members. Eligibility for the program was determined based on this index and a pre-determined threshold. Third, localities in the sample were randomly assigned to either the treatment (320) or control group (186). Eligible households in treatment communities were notified of their selection for the program and most of these families started receiving the benefits around May of 1998. Less than two years later, between November and December 1999, eligible households from the control communities were

[22] Marginalization was defined using a pre-determined locality-level index generated every five years by the Mexican Ministry of Population. This index combines several locality's characteristics. Rural communities are defined as those below 2,500 inhabitants.

incorporated into the program (Skoufias et al., 2000; Coady, 2000; Fernald and Gertler, 2005).

1.7.2 Empirical identification of *Progresa's* effects

Randomized Experiment. Most of the previous work related to *Progresa* has taken advantage of the randomization aspect of it. Behrman and Todd (2000) showed that several basic variables such as age, gender, income, and schooling are balanced when comparing households in control and treatment localities. The first empirical estimation used here to assess the potential mitigating effects of *Progresa* follows the line of randomized control trial's estimations:

$$Y_j = \eta_1 rain\_shock_j + \eta_2 Treat_j + \eta_3 rain\_shock_j * Treat_j + \epsilon_j \tag{1.2}$$

where Y_j represents the outcomes (cognitive and anthropometric) aggregated at the village level; $rain_j$ represents the dummy variable indicating the occurrence of the weather shocks; and $Treat_j$ indicates if village j was randomly selected as a treatment locality.

In the specification, η_2 represents the effect of the early treatment on the outcomes at villages not exposed to rain shocks, and $\eta_2 + \eta_3$ represents the effect of the early treatment in villages affected by the shocks. In this estimation, it is important to keep in mind that the control villages were added to the program in late 1999, less than two years after the treatment villages. Therefore, the randomization makes possible to identify early versus late treatment effects, rather than the more traditional treatment versus control effect.

Regression Discontinuity (RD). To implement a regression discontinuity design, we take advantage of the administrative rule that determines eligibility based on the household poverty index and a pre-determined cutoff. At the beginning of the program, 41 geographical regions were defined. Regions differ from each other on the weights attributed to variables used to generate the poverty index, and the cutoff value to select beneficiaries. A standardized poverty index (x_pmt_j) is formed by subtracting the regional cutoff to the each household's poverty index. Therefore, independently of the region, a household would be eligible for the program if its standardized poverty index is above zero ($x\_pmt_j > 0$).

The main assumption behind the RD strategy is that, other than the treatment benefits, the households around the cutoff value are comparable to each other. Therefore, any discrete change in an outcome variable occurring at the cutoff point can be related to the effect of the treatment (Imbens and Lemieux, 2007).[23]

RD methods can employ both, parametric and non-parametric estimators. However, the best way to illustrate the RD is with graphical analysis. We organized the analysis in two parts:

1. First Stage: we begin by showing the discontinuity of *Progresa's* beneficiaries at the administrative cutoff. We use the sample to estimate: $E(Benef_j|x\_pmt_j)$, where $Benef_j$ is a dummy variable equal to one if household j is a *Progresa* beneficiary. If targeting of the program and compliance were perfect we would expect to have a sharp RD.

2. Reduced Form: we show the conditional means of the outcome variables with respect to the standardized poverty index $[E(Outcome_{ij}|x\_pmt_j)]$. Any discrete jump at the cutoff value is attributed to the treatment. To estimate the potential mitigating effects of *Progresa* against the weather shocks, this analysis is performed for two subsamples: those observations living in villages affected by the rain shocks, and those not.

1.7.3 Results on the potential mitigation effects of Progresa

Randomized Experiment. *Table 1.8* presents the intent to treat (ITT) estimates of *Progresa* differencing between villages that suffered a weather shock and those that did not. The evidence from the tables suggests that there is neither a mitigation nor a direct effect from *Progresa* on the anthropometric and cognitive outcomes analyzed in this paper. To produce this analysis, data was

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[23] To assess the effectiveness of the RD method, the authors estimated the effect of *Progresa* on school attendance in 1999 of children between 6 and 15 years old (the age groups whose attendance is part of the conditionality to receive the monetary benefits). The RD estimates a 5 percentage point, statistically significant, increase in the likelihood of school attendance at the cutoff for those children in treatment villages. No discrete change is observed for children in control villages. Furthermore, after the cutoff, the level of school attendance for control and treatment villages follows different trends (graph available upon request).

aggregated at the village level given that both, the randomization and the identification of the weather shocks, were at the village level.[24]

Table 1.8: The mitigating effect of Progresa in villages exposed to the rainfall shock. These results are associated to the anthropometric, health, and cognitive development indicators collected in 2003. Coefficients are estimated using the randomized experiment empirical specification (equation 1.2). Outcomes in this model correspond to village level means of individual observations.

Anthropometric and Health Indicators

	weight (lb) (1)	height (in) (2)	Stunting (3)	Anemia (4)	Days_sick (5)
Rain shock[a]	-0.516	-0.467**	0.126***	0.0297	0.0847
	[0.4104]	[0.2298]	[0.0357]	[0.0254]	[0.1768]
Treatment[b]	-0.181	-0.0598	0.0118	0.0510*	-0.0997
	[0.4066]	[0.2182]	[0.0303]	[0.0307]	[0.1689]
Treatment x Rain shock	-0.702	-0.255	0.00271	-0.0407	0.0485
	[0.5705]	[0.3153]	[0.0505]	[0.0374]	[0.2321]
Observations[c]	259	259	259	258	259
R^2	0.06	0.06	0.09	0.01	0.01
Mean	33.92	38.29	0.301	0.261	1.380

Cognitive Development Indicators (log)

	Peabody Test	**Woodcock-Muñoz Test**		
	language (1)	long term memory (2)	short term memory (3)	visual-spatial thinking (4)
Rain shock[a]	-0.159**	-0.137**	-0.100**	-0.139***
	[0.0762]	[0.0684]	[0.0406]	[0.0439]
Treatment[b]	0.0330	-0.00983	-0.0419	-0.0459
	[0.0724]	[0.0650]	[0.0402]	[0.0452]
Treatment x Rain shock	-0.0197	-0.0463	0.0592	0.0466
	[0.0976]	[0.0864]	[0.0556]	[0.0606]
Observations[c]	253	259	259	259
R^2	0.05	0.06	0.03	0.06

Robust standard errors [in brackets]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
[a] *rain_shock* $= 1$ if village had a flood occurrence in 1999.
[b] *Treatment* randomly defined at the village level.
[c] Outcomes are village level means of individual observations.

In previous work, (Neufeld et al., 2005) find positive effects from *Progresa* on anthropometric outcomes when comparing children in experimental villages to children from a synthetic control (formed from villages that by 2003 were still not receiving *Progresa's* benefits).[25] Moreover, as in these estimates, they don't find differences between children in the original treatment and con-

[24] Similar results are obtained if the estimates are calculated at the household level.

[25] The synthetic control was selected using matching estimators.

trol villages. They argue that children in the original control villages catch-up with children that received the benefits earlier. The key assumption behind their main results is that the synthetic control villages had to be similar to the experimental villages in 1997. However, this is a strong assumption given that *Progresa* targeted the most disadvantaged localities by design. By the beginning of 2003, *Progresa* had geographical presence in 2,354 municipalities (97% of Mexico's total municipalities).

Rather than following this approach, this paper exploits the rule that determines household eligibility based on the poverty index and the pre-determined cutoffs. This gives the ideal setup for a regression discontinuity analysis.

Regression Discontinuity. *Figures 1.2* to *1.4* show the main results from the RD analysis. The first set of graphs (*Figure 1.2*) show the First Stage results described in *Section 1.7.2*, which justify using of RD to identify the effects of *Progresa*. These graphs show the evolution of the likelihood to be a *Progresa* beneficiary, conditional on the standardized poverty index (x_pmt). As expected form the program's rules, there is a discrete discontinuity exactly at the cutoff level, equal to 46.5 percentage points (according to a parametric estimate). The discontinuity persists and does no change much until 2002. Between late 2001 and early 2002, the program was expanded and the models to estimate the poverty index changed, thus explaining the lack of discontinuity in 2002.

The analysis is restricted to treatment localities. If control localities were added, the shape of the graphs in *Figure 1.2* would change in early 2000, when the control villages began to receive *Progresa's* benefits. By restricting the analysis to treatment villages, we have a discontinuity that remains close to constant until 2001. Therefore, the RD estimates give the difference between receiving the treatment from 1998 rather than from late 2001 at the discontinuity point. Given that the outcomes analyzed on this paper were measured in 2003, we believe that the RD approach should allow a better identification of the *Progresa* effects. This approach gives a lower time window for households that receive the benefits later to catch-up with those that received them form the beginning of the program. Also, the RD assumptions are less restrictive than those required to

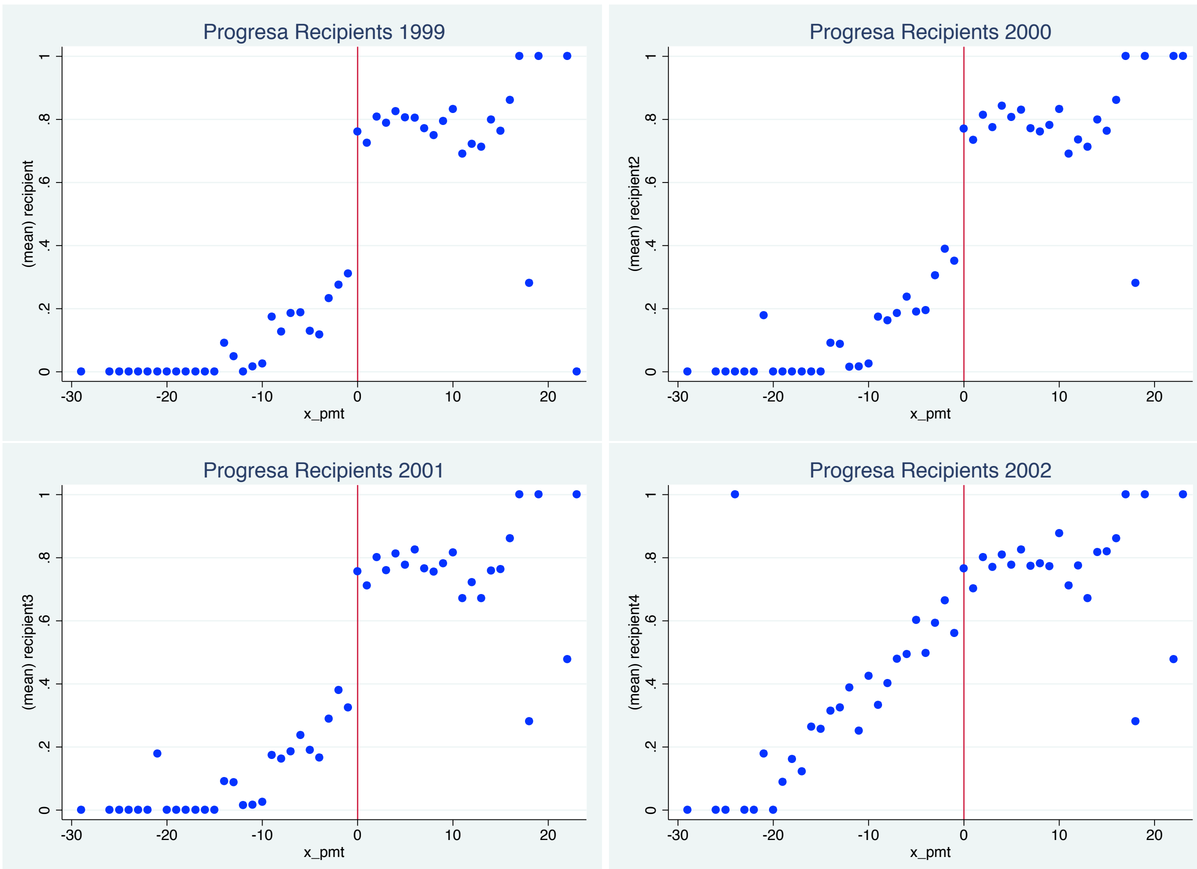

Figure 1.2: Regression Discontinuity: First Stage ID

On each graph, the x-axis corresponds to the standardized poverty index used by the administrative rule to select Progresa beneficiaries. The administrative cutoff is centered at zero.
The standardized poverty index (*x_pmt*) is formed with a formula that weights household's asset ownership and socio-economic characteristics of its members.
Analysis restricted to original randomized treatment villages.
The y-axis gives the proportion of households that report receiving the cash transfers of the program. Perfect targeting and take-up rates would yield a sharp regression discontinuity on the 1999-2001 graphs

use *Progresa's* 2003 synthetic control.[26]

Figure 1.3 illustrates the result of the RD analysis for two anthropometric outcomes: weight and height. Similarly, *Figure 1.4* gives two examples using cognitive outcomes: language (PPVT test) and long-term memory (Woodcock-Muñoz test). The triangles (circles) in the graphs represent the conditional means for those children that were (not) affected early on childhood by the ENSO-related shocks. The RD graphical analysis for the rest of the anthropometric, health and cognitive outcomes is included in *Figures A.2* and *A.3* in the supplementary material. The difference in the level of the means for the two subgroups reflects the negative effect of the shock, which is consistent with *Section's 1.5* analysis. However, *Progresa* does not seem to provide mitigation effects against the shocks (nor even direct effects on the outcomes).

The results are surprising given that previous research has shown positive effects of *Progresa* on food consumption and diet composition (Behrman and Hoddinott, 2005; Hoddinott et al., 2000; Vicarelli, 2011). Applying the RD analysis to the consumption and diet composition indicators analyzed in this paper, we find positive, but modest effects at the discontinuity point. However, the positive changes on these indicators do not allow for a mitigation of the negative effects of the rain shocks.[27] Other possible explanation for a lack of mitigation evidence includes differences in intra-household allocation of resources. Previous work has shown that when facing negative weather income shocks, children are the most affected in terms of consumption. Baez and Santos (2007) give evidence that after hurricane Mitch hit Nicaragua, children's likelihood of being undernourished significantly increased, while adult's consumption wasn't reported to be greatly affected. In the case of *Progresa* there is also a higher incentive to protect children at school age, given that the amount of cash transfers significantly increases with school attendance of 8 to 15 year old children (i.e. children attending 3rd to 6th grade of primary or lower secondary). Finally, the negative conditions that result from the exposure to weather shocks might have led to stress. There is a growing literature that gives evidence of negative effects of early-life exposure to stress

[26] As described previously, Neufeld et al. (2005) and Fernald and Gertler (2005) use the 2003 synthetic control. Also, several other *Progresa* medium-term evaluations adopted the 2003 synthetic cohort approach.

[27] Graphs can be made available upon request.

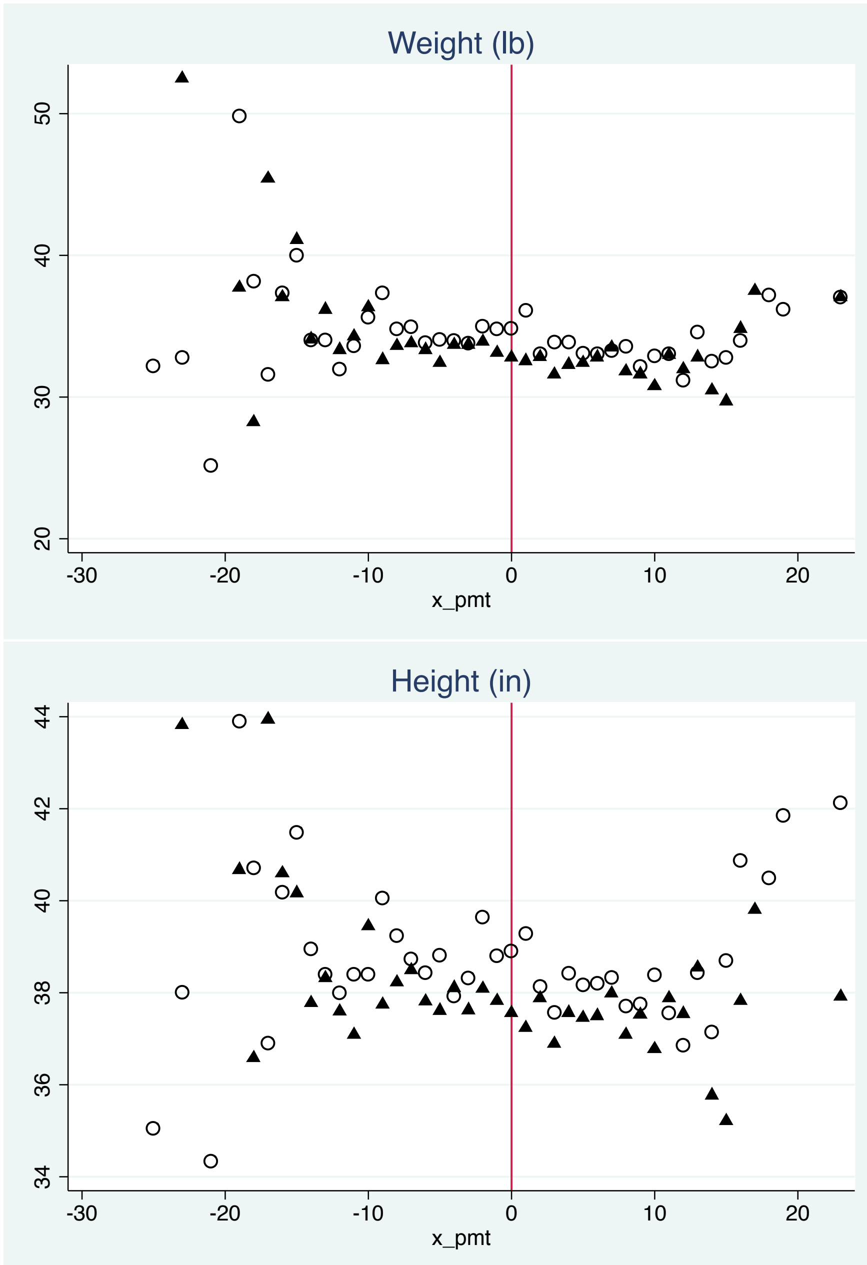

Figure 1.3: Regression Discontinuity Analysis: Anthropometric Outcomes

On each graph, the x-axis corresponds to the standardized poverty index used by the administrative rule to select *Progresa* beneficiaries. The administrative cutoff is centered at zero.
The standardized poverty index (*x_pmt*) is formed with a formula that weights household's asset ownership and socio-economic characteristics of its members.
Analysis restricted to original randomized treatment villages.
The y-axis gives conditional means of the individual outcomes. ▲is the conditional mean for individuals from villages affected by a rain shock. ○is the conditional mean for individuals from villages not affected by a rain shock.

Figure 1.4: Regression Discontinuity Analysis: Cognitive Outcomes

On each graph, the x-axis corresponds to the standardized poverty index used by the administrative rule to select *Progresa* beneficiaries. The administrative cutoff is centered at zero.
The standardized poverty index (*x_pmt*) is formed with a formula that weights household's asset ownership and socio-economic characteristics of its members.
Analysis restricted to original randomized treatment villages.
The y-axis gives conditional means of the individual outcomes. ▲is the conditional mean for individuals from villages affected by a rain shock. ○is the conditional mean for individuals from villages not affected by a rain shock.

on later physical health, cognitive abilities, and educational outcomes (Ecclesston, 2011; Kaiser and Sachser, 2005).

It is important to indicate that a limitation associated with the RD estimates is that it provides a Local Average Treatment Effect (i.e. the effect of the treatment around the cutoff level). If the treatment has heterogenous effects along the income distribution, then this result cannot be generalized to the rest of the population. It could be argued that the effects of *Progresa* are stronger for the poorest populations. However, given the sample characteristics and *Progresa's* design to target the poor, it would be expected that the group around the cutoff to be already representative of poor (although not extreme) Mexican rural households.

1.8 Conclusions

Previous work has shown that the early-life conditions tend to have a strong influence on an individual's life. Economists' work has analyzed impacts on income, educational attainment, health, and even mental and physical disabilities (Almond 2006; Almond and Mazumder 2011; Maccini and Yang 2009). This paper contributes to the literature by estimating the medium-term impact that early-life negative conditions have on specific aspects of children's health and cognitive development. Scores of highly reliable tests (according to U.S. standards) inform about specific abilities that are negatively affected, namely, language, long-term memory, and visual-spatial thinking. Objective anthropometric measures, like height, are also negatively altered. These indicators have been shown to be strong predictors of school and later in life success. Hence, the paper provides information about specific channels that might be driving the long-term effects previously encountered. According this study, income, consumption and diet composition at early life stages are key mechanisms that contribute to produce these results.

Weather shocks related to "El Niño Southern Oscillation" are used to identify negative conditions at early life stages. ENSO is a recurrent climatic event with global impacts that affects hydrometeorological patterns, causing extreme weather events (e.g. floods, heat waves, droughts). With global warming, extreme weather events are expected to increase in frequency and intensity. Therefore, findings about Mexico are relevant for households in other developing countries with comparable climates, and affected by ENSO-related weather events (e.g. Africa, Latin America, South-East Asia). The analysis of its effects is relevant from an economic, climatic, and public policy perspective.

Finally, no mitigation of *Progresa* against the negative effects of weather shocks has been found. Some potential reasons are: (i) *Progresa* did not completely mitigate the negative effects of the weather shocks on consumption and diet composition; (ii) intra-household imbalance in the distribution of *Progresa's* resources; (iii) other components related to the weather shocks, like stress, might be contributing to the results and are not offset by *Progresa*. In future work, we plan to assess the second point to determine if intra-household allocation of resources could explain the no-effect result found for *Progresa* in this and previous studies (Fernald and Gertler 2004). Heterogeneity in the effects of *Progresa* with respect to children's initial malnutrition is also on our agenda.

2. THE MEDIUM-TERM IMPACT OF A CONDITIONAL CASH TRANSFER PROGRAM ON CHILD PHYSICAL AND COGNITIVE DEVELOPMENT: EVIDENCE FROM *PROGRESA*

"The family is the major source of inequality in American society, in most societies"

- James Heckman

2.1 Introduction

Early life stages are recognized as critical towards human development. Physical and cognitive development are highly sensitive during this period (Shonkoff and Phillips, 2000). These qualities have been proven to be strong predictors of later schooling and life success (Breslau et al., 2001; Currie and Thomas, 2001; Nikolov, 2011). Tying these pieces together, a growing body of research has shown that conditions early on life (in-utero and during the first years of life) tend to have long term consequences on various socio-economic indicators (Almond, 2006; Almond and Mazumder, 2011; Case and Paxson, 2008; Maccini and Yang, 2009). Therefore, interventions that attempt to benefit children born (or to be born) on disadvantaged settings are relevant to compensate for their initial conditions.[1] Failing to correct these initial inequalities might result in a persistent (or even divergent) gap in various socio-economic dimensions throughout life (Cunha and Heckman, 2006; Heckman, 2006). In developing countries, the exposure of children to poverty, malnutrition, poor health, and unsupportive home environments make of this problem a great concern (Grantham-McGregor et al., 2007).

[1] See Cunha and Heckman (2006) for a review of early childhood intervention programs in the U.S. Examples of programs targeting preschool children that have been implemented in the developing world include: providing nutritional supplements and stimulation to 9-24 month old stunted children in Jamaica (Grantham-McGregor et al., 1991, 1997; Walker et al., 2000); and nutritious food supplements on villages with high incidence of malnutrition in Guatemala (Maluccio et al., 2009).

Conditional cash transfer programs (hereon CCTs) have become increasingly popular policies to fight poverty transmission and inequality. Generally, CCTs' main focus is the promotion of health, nutrition, and schooling, mainly of young household members. In addition, CCTs provide cash transfers to ease poor household's credit constraints.[2] A vast literature exists showing the positive impacts that CCTs have had in several dimensions of poor beneficiaries (see Parker et al. (2008) for a review of the literature). However, few papers have analyzed whether CCTs amend (at least partially) inequalities that arise early in life. If these initial gaps are not corrected, the benefits that CCTs have been shown to provide, like health and education, might not be sufficiently effective to close the socio-economic disparities.

This paper investigates the medium-term effects of exposure during early stages of life to the PROGRESA-Oportunidades CCT program[3] (hereon *Progresa*) on children's physical, cognitive, motor skill, and behavioral development. It will also suggest a methodology to isolate the contribution of the cash transfers from that of the conditionality components of the program.[4] Moreover, this paper will provide a rigorous estimation to test the results recently disseminated which concluded that increases in cash transfers generate improvements in children's physical and cognitive characteristics (Fernald et al., 2008; Manley et al., 2012).

In previous work that analyzed *Progresa's* medium-term effects during early childhood, Neufeld et al. (2005) found positive effects in height by comparing the original experimental localities with observations from new control localities added in 2003.[5] No effects were found using the exper-

[2] CCTs generally consist on cash transfers delivered to poor households conditional on compliance with a set of human capital investment requirements. The conditions apply mostly to children and range from mandatory educational enrollment, regular health monitoring to pre- and post-natal care (Fizbein and Schady, 2009).

[3] Mexico's PROGRESA-Oportunidades is the most widely known CCT program because of the academic dissemination of its results. Its data is a panel collected in several waves between 1997 and 2008. The data is publicly available and covers various topics. PROGRESA-Oportunidades has had a big impact towards CCTs expansion to other countries (Mexican Ministry of Development, 2012a).

[4] PROGRESA-Oportunidades cash transfers are conditional on children's school attendance and household members attendance to health check-ups; parents are required to attend community meetings where information about good health practices is distributed; and pregnant women have to attend to at least five medical appointments (Diario Oficial de la Federación, 2002)

[5] The original control localities began to receive treatment a year and a half later than treatment localities, making the

imental localities and the initial randomization. The authors argue that since children from the control localities began to receive the program just one year and a half after the original treatment, they catch-up in anthropometric development. Using a similar strategy, Fernald and Gertler (2005) found positive medium-term effects on motor skill development, but none on cognitive abilities.

Recently, a group of papers has attempted to isolate the effect of the cash component on early child development in some of the dimensions before mentioned. Fernald et al. (2008) begin by claiming that cash transfers are associated with improved physical, cognitive, and motor skill development using *Progresa* data. They identify the effects by using a linear estimation of the accumulated cash transfers received by the household on children's outcomes. The sample is restricted to children living in households that have received cash transfers at least one month before they were born. Given this restriction, they argue that the results reflect the association between cash transfers and outcomes, since all the children had been exposed to the conditionalities. In a follow-up paper, Manley et al. (2012) found similar results using potential cash transfers[6] as instrument for actual transfer amounts received.

In related work, Paxson and Schady (2010) used a randomized intervention at the local level in Ecuador and found modest but positive effects of *Bono de Desarrollo Humano* program's cash transfers on children's physical, cognitive and socio-emotional development (with the poor being more benefited). Macours et al. (2012) used a Nicaraguan randomized intervention, *Atención a Crisis*, that distributed cash and child-care information on households with children aged 0-5. They found positive effects on cognitive development 9 months after the initial treatment and up to two years after the program ended. Further evidence from their study suggests that the effects are mainly due to the information distributed to households rather than the cash component.

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initial randomization and early versus late treatment comparison. In 2003, 151 new localities were added to serve as an artificial control. These localities are located in the same States as the original experimental localities, but by 2003 they had not been added to the program. Propensity score matching methods based on locality observable characteristics were used to find similar localities to the original ones.

[6] The potential transfers are estimated based on the program's rules and each household's demographic composition, randomly given treatment status, and children's school attendance.

This paper employs data from the *Progresa* evaluation surveys. Five years after *Progresa's* initial randomization, a detailed follow-up survey was collected in 2003. This survey includes objective indicators of anthropometric, cognitive, motor skills, and health development from children aged 2 to 6. The data is longitudinal and it can be related to previous surveys, including the 1997 baseline, the 1998-2000 bi-annual follow-ups, and *Progresa's* administrative information about cash transfers.

The main findings in this paper contrast with previous results from the literature. First, using the original randomization localities, the average effects of being born in an early-treatment locality (original treatment) with respect to a late-treatment locality (original control) are estimated.[7] Birth at early-treatment localities would provide more exposure to health care, plus cash transfers on average $483 and $530 Mexican pesos higher during pregnancy and first year of life,[8] respectively (these amounts are equivalent to a 6.9% and 6.3% increase in the value of household's food consumption, respectively). No advantage of being born in an early-treatment locality is found in any of the dimensions analyzed.

Second, the paper employs the random difference of the phase-in of the original localities (April 1998 versus November 1999) and the children's date of birth to investigate if there are medium-term effects from exposure to the program during different stages of early child development. For example, children born in early-treatment localities between January 1999 and October 1999 would have been exposed to the program during all their in-utero development. No conclusive evidence of benefits in any of the dimensions is found using this approach.

Third, this paper evaluates if the no effect results are explained because the children from the late-treatment localities catch-up with those in early-treatment as argued in Neufeld et al. (2005). A regression discontinuity (RD) design is implemented using *Progresa's* eligibility rule based on a

[7] The original treatment localities began to receive the benefits or the program in April 1998 and the control localities in November 1999. The outcomes analyzed in this paper are collected between September and November 2003.

[8] Transfers during pregnancy are those received during the ten months previous to the child's birth. Transfers received during the first year of life are those received during the 12 months after the child's birth (including the month of birth)

proxy means test. According to *Progresa's* rules, once a locality is added to the program, households are not assessed for inclusion/removal until three years after.[9] By restricting the observations to treatment localities, it is shown that the proxy means eligibility discontinuity remains from the start of the program (April 1998) until three years after. No benefits from the program are found by comparing the outcomes of children just before and after the discontinuity in the proxy means score. This result contrasts with the catch-up hypothesis.

Finally, the paper investigates the isolated effects of increases in cash transfers. This method takes advantage of discrete changes for the educational cash transfers specified in *Progresa's* rules. A large increase occurs between 2nd and 3rd grade, where the transfer changes from $0 to $70 Mexican pesos per month (April 1998 cash transfer amounts). By restricting the sample to preschool children living in households where the oldest sibling's age is such that he/she should be just before or after 3rd grade, two groups are constructed. An exogeneity test shows that, other than differences in cash transfers, these two groups are similar in terms of baseline observable characteristics (except for parents age and household size). Cash transfer increases estimated are equal to $158 and $344 Mexican pesos during pregnancy and first year of life. No conclusive evidence of impacts of increasing cash transfers on medium-term physical, cognitive, and motor skill development are found.

Overall, the results give weak evidence of medium-term effects on preschool children's anthropometric, cognitive, and motor skill development for exposure to *Progresa* during early stages of life. *Progresa* does not seem to correct considerable initial disadvantages of children born on poor settings. If the initial disadvantage results in lower returns to human capital investments, then the findings of this paper suggest that *Progresa* could be less effective reducing poverty and inequality.

The remainder of the paper is organized as follows: *Section 2.2* provides some context about *Progresa*; *Section 2.3* describes the data used; *Section 2.4* details the empirical specifications; *Section 2.5* presents the results; and, finally, *Section 2.6* concludes.

[9] This was done to avoid households from close-by localities to migrate to recipient localities in order to be added to the program.

2.2 *Progresa* Description and its Effects on Early Childhood

Mexico's PROGRESA-Oportunidades program is a basic reference among CCT programs. *Progresa* was created with the purpose of "supporting poor households to foster the capacities of their members and expand their alternatives to reach higher levels of wellbeing by improving their options to access education, health and nutrition[10] (Diario Oficial de la Federación, 2002)." Its strength lies in a solid institutional foundation and a rigorous evaluation design that makes it possible to objectively assess its results under high standards (Levy, 2006).

2.2.1 General description

Progresa started in August 1997. Nowadays, it is the most comprehensive poverty reduction program in Mexico. By 2012, it reached a coverage of 5.8 million households (23% of the Mexican households) and it is expected to be extended to 6.5 million within the next years (Mexican Ministry of Development, 2012b). For 2012, the approved budget for the program amounts 63.9 billion Mexican Pesos (0.4% of 2011 Mexico's GDP) (Diario Oficial de la Federación, 2011).

Between 1997 and 2000, while the program was being expanded at a national level, a randomized evaluation design was implemented in a subsample of 506 localities that were initially determined as eligible to receive the program. Of the 506 localities, 320 were randomly designed as treatment and 186 as control. The purpose of the experiment was to rigorously estimate the impact of the program on several dimensions, giving *Progresa* a high academic exposure (Fizbein and Schady, 2009).

2.2.2 Components and conditionalities

At the time *Progresa* began, it consisted of three main components:[11] (i) **education**, that was promoted by providing cash transfers to households for each child enrolled and regularly attend-

[10] Author's translation of the original *Progresa's* main objective.

[11] After 2006, additional components were added to the program. This components increased the lump-sum transfers given to the households without establishing additional conditions, except for the elderly people component that is conditional on having a household member over 70 years old present in the household.

ing school (at least 85% of turnout); (ii) **nutrition**, that consisted on lump-sum cash transfers[12] and delivery of food supplements (targeted to children and lactating or pregnant women) given to households complying with the health conditionality and attendance to information sessions; and (iii) **health**, that consists of regular check-ups directed to all household members, but with a special emphasis on children under 5 years old and pregnant or lactating women. Also, the female household head is required to attend regular sessions that distribute information about good health care practices (Hernández et al., 1999).

Cash transfers (educational and nutritional) are delivered to the female head member of the household every two months. Families receive information suggesting them how to use the transfers in order to improve the conditions of its members. However, in practice, households can freely decide how to spend the resources.

To become a *Progresa* beneficiary, the household has to fulfill the following conditions:[13]

1. Reside in a locality that has been declared as eligible to receive *Progresa*. Preference was given to the most marginalized localities.[14] Selection was restricted to rural localities (below 2,500 inhabitants) that have access to school and health services (the conditionalities) (Cruz et al., 1999).

2. Qualify as an eligible household. Eligibility is identified by proxy means test using information collected with a Census in the selected localities. The proxy means test combines household's asset ownership, characteristics of the household head, and household demographic characteristics (Hernández et al., 1999).

3. Attend to the locality meeting that assembles all eligible households to complete their enrollment. In this meeting households receive documentation and guidelines of how to meet

[12] Upon delivery of resources, it is suggested to families to use part of the transfers to improve the diet and nutrition of the household members, particularly female and children.

[13] These conditions correpond to the requirements that were valid between 1997 and 2003 when only rural localities (below 2,500 inhabitants) were eligible to receive *Progresa*. After 2003, urban localities were included in the program and some of the conditions were modified.

[14] A marginality index is calculated every five years by the National Institute of Geography and Statistics (INEGI). The index is obtained through a weighted linear combination of socio-economic indicators at the locality level.

the conditionalities and receive the cash transfers[15] (Hernández et al., 1999).

2.2.3 Background on early childhood benefits of *Progresa*

Some of *Progresa's* components are intended to directly benefit children at their early stages of development. Women during pregnancy are required to attend at least five checkups and receive nutritional information, as well as iron and nutritional supplements. After delivery, mothers are required to have two additional checkups in which they receive information about child rearing, breastfeeding and family planning. Also, children below the age of five are required to attend health checkups (more frequently than a normal household member) to receive immunizations, early detection of child-common sickness, growth and nutrition assessments, and nutritional supplements (Hernández et al., 1999).

Previous work has found positive impacts of *Progresa* on children's development during their early stages of life. Utilization of health infrastructure increased (Gertler, 2000). Benefits during the pregnancy stage have been shown to result in increments of birth weight (127.3 grams on average) and a decrease in low birth weight incidence (44.5 percent) (Barber and Gertler, 2010). The effect seems to be greater for children of higher percentiles of birth weights (Flores-Martinez, 2010). Also, an 11% reduction of child mortality, being the effect more pronounced in more marginalized municipalities has been found (Barham, 2011).

Height is a common objective indicator to assess the effect of nutrition and access to care early in childhood. A first group of papers found positive effects of the program exposure for a subsample of children measured one to two years after the start of the program (Gertler, 2004; Behrman and Hoddinott, 2005). A later study that uses the 2003 *Progresa* follow-up survey, also finds differences on the medium run (Neufeld et al., 2005). However, this study compares only the outcomes of children in the experimental localities to those of children in new "control" localities. Finally, Farfán et al. (2011) find significant effects on height of children aged 5-8 when comparing children fully to partially exposed to the program from their birth using the Mexican Family Life Survey.

[15] In theory, during this meeting, members of the community can oppose to certain families being added to the program. In practice, objections were presented in less than 0.1% of the cases (Skoufias et al., 2000).

Finally, as mentioned in the *Introduction*, only two papers have investigated the effects of *Progresa* on children's cognitive development (Fernald and Gertler, 2005; Fernald et al., 2008), finding inconsistent results about the effect of the program.

2.3 Data

The main outcomes used in this study come from the 2003 *Progresa* follow-up survey. Five years after the initial randomization of the *Progresa* experiment communities, this later wave of data was collected to analyze medium-term effects of the program. Anthropometric, cognitive, health, motor skills, and behavioral information of children aged 2-6 was gathered from a sub-sample of the original 506 villages.[16] The sample for the analysis is restricted to children from eligible households that have an available date of birth.[17] This results in a sample of 247 villages, 2,049 households and 2,585 children that is used in the analysis. Using the longitudinal component of the *Progresa* databases, the information can be related to the baseline (1997) characteristics of each children's households as well as their parents' characteristics. *Table 2.1* includes some descriptive statistics of these indicators.

Table 2.1: Descriptive statistics*

Variable	Num. obs	Mean	Std. Dev.	Min	Max
Outcomes: Anthropometric					
Height (Z-score)	1820	-1.85	1.0220	-4.65	1
Stunting (binary)	1856	0.44	0.4967	0	1
Weight (Z-score)	1871	-0.81	0.9611	-3.87	12.39
BMI (Z-score)	1819	0.57	0.8372	-1.69	3.77
Overweight (binary)	1855	0.15	0.3528	0	1
Outcomes: Cognitive tests					
LT memory (% correct)	2405	0.16	0.1446	0.01	0.65

Continued on next page

[16] Data is publicly available at http://evaluacion.oportunidades.gob.mx/evaluacion

[17] Date of birth was verified against self reported age for consistency. Whenever there was an inconsistency in the years, but not in months (assuming the month and day of birth are easier to recall than the year) the self-reported age is used to correct the year of birth.

Table 2.1 Descriptive statistics – continued

Variable	Num. obs	Mean	Std. Dev.	Min	Max
ST memory (% correct)	2292	0.37	0.1630	0.03	0.66
Visual-spatial (% correct)	1989	0.23	0.1072	0.02	0.50
Language (% correct)	1926	0.10	0.0795	0.01	0.38
Outcomes: Motor skills					
Balance (secs)	2336	7.58	5.0073	0	37.5
Walk back (binary)	2476	0.83	0.3714	0	1
Tiptoe (binary)	2411	0.74	0.4383	0	1
Walk straight (binary)	2458	0.77	0.4206	0	1
Jump (binary)	2232	0.27	0.4455	0	1
Outcomes: Health and behavioral					
Hemoglobin (g/dL)	2480	11.47	1.3403	7	14.3
Days sick	2248	1.18	2.3382	0	15
Depression (Z-score)	2448	-0.01	0.9749	-1.48	2.50
Aggression (Z-score)	2470	-0.10	1.0167	-1.76	2.30
Individual variables					
Age (months)	2585	49.59	13.4946	24	72
Male (binary)	2585	0.51	0.5000	0	1
Num siblings	2271	3.96	2.2524	0	13
Baseline variables (1997)					
Land owned (ha)	2585	1.44	2.4640	0	23
Water access (binary)	2585	0.27	0.4460	0	1
Draft animals (binary)	2585	0.32	0.4679	0	1
Small animals (binary)	2585	0.78	0.4139	0	1
Electricity (binary)	2585	0.66	0.4725	0	1
Poverty index (Z-score)	2585	0	1.5827	-1.07	16.32
HH Demographic characteristics (1997)					
% 0-5 years	2585	0.29	0.1641	0	0.67
% 6-17 years	2585	0.28	0.2098	0	1
% 18-49 years	2585	0.38	0.1472	0	1
% over 50	2585	0.05	0.1011	0	1
Household size	2585	6.17	2.3671	2	24
Parents' characteristics					
Head speak indig (binary)	2585	0.51	0.4999	0	1
Father present (binary)	2584	0.90	0.2930	0	1

Continued on next page

Table 2.1 Descriptive statistics – continued

Variable	Num. obs	Mean	Std. Dev.	Min	Max
Father yrs educ	2207	3.88	2.9098	0	17
Mother yrs educ	2267	3.74	2.8648	0	20
Mother age	2573	33.17	6.6672	18	72
Mother height (cm)	2572	147.91	4.9998	126	170.5
Mother language score	2568	75.80	19.3907	1	125
Random Treatment status					
Treatment (binary)	2585	0.58	0.4929	0	1
Cash transfer variables (Mx. Pesos)					
Monthly CCTs (Oct 98)	1134	213.17	151.724	47.97	731.04
CCTs during pregnancy	1349	1447.51	1083.373	9.79	4931.15
CCTs during 1st year	1580	1964.28	1354.804	9.79	6187.75
Total CCT (May 98-Jun 03)	2133	11867.58	7062.389	198.80	32873.09
Monthly baseline economic indicators (Mx. Pesos)					
Food expenditure	1036	472.24	235.2421	78.33	1370.70
Value of food consumed	1049	700.39	316.0029	201.04	2255.35
Val food cons (per capita)	1051	116.02	60.5961	25.22	374.78
HH monetary income	2468	777.36	629.6467	8.63	5180.00

* Sample restricted to children with available date of birth from eligible families living in localities where anthropometric and cognitive data was collected in 2003.

2.3.1 Anthropometric data

Height and weight were collected by trained personnel using regularly calibrated portable scales and stadiometers (Neufeld et al., 2005). These measures were standardized with respect to a healthy age-sex reference population following the methodology recommended by the World Health Organization.[18] Using the standardized measures two other indicators were calculated: (i) *stunting*[19] or low weight for age, which is a binary variable equal to *one* if the height is two or more standard deviations below the age-sex standardized height; and (ii) *overweight*, which is a binary

[18] World Health Organization software was used to generate the standardized values. Access to the software is publicly available at `http://www.who.int/nutgrowthdb/software/en/`

[19] Stunting usually reflects insufficient nutrient intake during early stages of development. It generally occurs before age two and once established, it is usually permanent. Possible consequences include delayed development, impaired cognitive function, and poor school performance (UNICEF, 2007).

variable equal to *one* if the body mass index (BMI) is above the *85th* percentile of the age-sex standardized BMI.

Table 2.1 shows that the group of children considered in the sample are on average 1.85 standard deviations below for height, 0.81 standard deviations below for weight and 0.57 standard deviations above for BMI with respect to the age-sex reference population mean. Stunting is prevalent in 44 percent and overweight in 15 percent of the sample. These indicators illustrate that serious lags in growth are prevalent in the sample used, probably as a result of undernourishment early in life. It is important to remember that the *Progresa* data was collected from Mexican marginalized rural communities and the sample was further restricted to children in eligible (poor) households.

2.3.2 Cognitive indicators

Objective measures for early child cognitive development are available in the *Progresa* dataset. These measures result from applying the *Peabody Picture Vocabulary Test* (PPVT) (Dunn and Dunn, 1986) and three subsections of the *Batería Woodcock-Muñoz Test* (WMT) (Woodcock and Muñoz-Sandoval, 1996). Both tests are acknowledged in the educational literature for their high *internal reliability* and *validity*.[20]

The PPVT measures the receptive vocabulary of children aged 3 to 6 by asking them to indicate which of four pictures best represents a stimulus word. Studies have found that vocabulary tests tend to be strong predictors of school success and contribute in a large extent on tests that assess general intelligence. The PPVT test is widely used with preschool children to assess early child development (Duncan et al., 2007).

Scores from three subtests of the WMT are available for children 2 to 6 years old. These subtests ask children to: learn associations between unfamiliar auditory and visual stimuli; remember

[20] In educational testing, *internal reliability* indicates the degree to which test scores for a group of test takers are consistent over repeated applications of the measurement procedure, and *Validity* refers to the degree to which accumulated evidence and theory support specific interpretations of the test scores (American Educational Research Association et al., 1999).

and repeat single words, phrases, and sentences; and identify an object's picture from a partial drawing or representation. The results are related to long-term memory, working memory, and visual-spatial thinking abilities, respectively.[21] The WMT has been used in the literature to evaluate the effect of early nutritional interventions on cognitive development and have been shown to detect differences between children with low birth weight incidence and those born with normal weight (Breslau et al., 2001; Lozoff et al., 1991).

The logarithmic transformation of the scores is used in the analysis. *Table 2.1* shows that on average children successfully answer 10% of the PPVT questions, 16% of the long-term memory, 37% of the short-term memory, and 23% of the visual-spatial integration portions of the WMT. Fernald and Gertler (2005) show that when compared to a standardized spanish-speaking population[22] these sample's average test results fall in the 18.9 percentile for the PPVT, and the 16.1, 21.5 and 7.2 for the three WMT subtests, respectively. These very low levels of cognitive development are distressing by themselves and give evidence of a big disadvantage that these children have after its early stages of development.

2.3.3 Motor skills

Motor skill indicators result from applying the *McCarthy Scale of Children's Abilities* (MSCA) to children aged 2-6 years old (McCarthy, 1972). Children are asked to perform a series of tasks that include: walk backwards, stand on one foot (twice, one for each foot), tiptoe, walk on a straight line (following a ribbon), and jump rhythmically alternating both feet. All this tasks are scaled in a three rank score depending in the level of achievement. Given that most children in the sample receive the highest score, the tasks are coded on a binary basis as successfully (if the highest score is received) or unsuccessfully completed (if the lowest two scores are received). The only exception is the indicator for *standing in one foot*, where the seconds endured on each foot are averaged

[21] Schrank et al. (2005) describe these abilities as follows: (i) long-term memory is the ability to store information and fluently retrieve it later; (ii) working memory (also referred to as short-term memory) is the capacity to hold information in immediate awareness while performing a mental operation on the information; and (iii) and visual-spatial thinking is the ability to perceive, analyze, synthesize, and think with visual patterns, including the ability to store and retrieve visual associations.

[22] The reference spanish-speaking population used to standardize the Woodcock-Muñoz results is obtained from a sample of 802 children from Costa Rica, Peru, Mexico and Spain.

to create a *balance* indicator. The MSCA is employed in the literature to measure mental competence and motor skill abilities (Black and Powell, 2004). Deficiencies in gross motor coordination (e.g. poor balance, poor timing and coordination, difficulty combining movements into controlled sequences) may reflect neuromotor and executive-function deficits (Poltajko et al., 1995).

Table 2.1 shows that, on average, children are able to hold balance for 7.8 seconds and are successful in the rest of the tasks 83% for walking back, 74% for tiptoe, 77% to walk straight, and 27% to skillfully jump.

2.3.4 Health and behavioral

Blood samples were gathered for children aged 2-6 years old. Hemoglobin levels were obtained from the samples and adjusted for village altitude to use as indicators for the prevalence of *anemia* (Ruiz-Argüelles and Llorente-Peters, 1981). High levels of hemoglobin are usually an indicator of poor nutrition (mainly iron deficiency) and poor health. Its negative consequences range from lower cognitive and physical development to increased risk of mortality (World Health Organization, 2008). Mothers are asked to self-report the number of days that each child was sick and unable to perform his regular activities during the past 4 weeks. Finally, two measures of behavioral attitudes (depression and aggression) are estimated using responses from mothers about their children's attitudes based on the *Achenbach Child Behavioral Checklist* (CBCL) (Achenbach and Rescorla, 2000).

Table 2.1 shows that on average hemoglobin levels are high, resulting in a 35% incidence of anemia in the sample. Average sick days reported are only 1.2 on average. Finally, the *depression* and *aggression* indexes calculated using the CBCL are standardized and reported in terms of standard deviations from the sample mean.

2.3.5 Cash transfers

All the households from the *Progresa* surveys can be related to administrative information that contains details about the cash transfers. Date of enrollment to the program and amounts transferred each two months are available for each household from 1997 up to February 2012. *Table 2.1* shows that at the beginning of the program (September 1998) the monthly cash transfer averaged \$213 Mexican pesos, which is equivalent to 43% of eligible household's food expenditure and 26% of household's monetary income.

Using the cash transfer information, three variables that will be used in the analysis were formed: (i) CCTs during pregnancy (CCT_preg) which is equal to the sum of the cash transfers received at the household level during the 10 months previous to the child's birth; (ii) CCTs during the first year of life (CCT_fstyr) which is equal to the sum of the cash transfers received at the household level during the 12 months after the child's birth (including the month of birth); and (iii) total accumulated cash transfers ($Total\_CCT$) which is equal to the sum of cash transfers received from the date of the household's enrollment up to June 2003. All values are discounted to January 1998 values using Mexico's CPI (Banco de México, 2012a). *Table 2.1* shows the average values for these variables as well.

2.4 Empirical Specification

To investigate if *Progresa* had medium-term effects on children's anthropometric, cognitive, motor skills, health, and behavioral development indicators collected in 2003, the following specifications are estimated.

First, the initial randomization is employed. The 506 villages were randomly assigned a *treatment* or *control* status. Households in *treatment* localities began to receive the benefits from the program in April 1998 and those in *control* localities were added to the program a year and a half later in November 1999. *Table 2.2* gives evidence that children born in *treatment* and *control* localities are similar in terms of their household's baseline characteristics. Therefore, the *treatment* indicator will give the difference in each children's (or their families') exposure to the program. Restricting our sample of children to those eligible to receive the program,[23] the following estima-

[23] As described in *Section 2.2.2*, eligibility was determined based on a poverty index calculated with the baseline (1997) information.

tion is calculated:

$$Y_{ij} = \phi Treat_j + \beta X_{ij} + \nu_j + \epsilon_{ij} \tag{2.1}$$

where Y_{ij} is the outcome for child i in locality j, $Treat_j$ is an indicator for locality j being assigned as *treatment* locality, X_{ij} are controls for child i in locality j, and ν_j gives locality-clustered standard errors.

Table 2.2: Exogeneity tests for treatment randomization using baseline (1997) data

Variable	Mean $Treat_j = 0$	Mean $Treat_j = 1$	Difference	t-statistic
Home characteristics				
Home owned	0.918	0.930	-0.0119	-0.967
Land owned	0.851	0.843	0.00765	0.453
Dirt floor	0.782	0.758	0.0241	1.218
Water access	0.226	0.307	-0.0808***	-3.888
Electricity access	0.676	0.663	0.0127	0.577
Asset ownership				
Blender	0.179	0.142	0.0364*	2.134
Refrigerator	0.0242	0.0349	-0.0106	-1.321
Gas stove	0.135	0.143	-0.00792	-0.487
Heater	0.0166	0.0211	-0.00452	-0.703
Radio	0.532	0.513	0.0190	0.814
Stereo	0.0293	0.0248	0.00457	0.605
TV	0.358	0.267	0.0914***	4.259
Video player	0.0115	0.00550	0.00598	1.432
Washer	0.0102	0.00550	0.00470	1.165
Fan	0.0293	0.0119	0.0174**	2.712
Car	0.00510	0.00183	0.00327	1.235
Van	0.0166	0.0101	0.00649	1.232
Draft animals	0.296	0.320	-0.0243	-1.120
Other animals	0.781	0.765	0.0155	0.787
Family characteristics				
Poverty index	632.3	637.5	-5.216	-1.354
Household size	6.014	5.926	0.0883	0.860
Num siblings	4.143	4.240	-0.0975	-0.980

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Continued on next page

Table 2.2 – continued

Variable	Mean $Treat_j = 0$	Mean $Treat_j = 1$	Difference	t-statistic
Monthly income (MxP)	726.3	737.9	-11.63	-0.324
Land ownership (ha)	1.384	1.386	-0.00231	-0.0203
Parents characteristics				
Mother spk indig	0.485	0.502	-0.0171	-0.732
Mother spk spanish	0.936	0.944	-0.00781	-0.706
Fahter spk indig	0.499	0.529	-0.0306	-1.309
Father spk spanish	0.976	0.968	0.00788	1.005
Father years educ	3.578	3.841	-0.263*	-2.098
Mother years educ	3.577	3.483	0.0940	0.747
Father age	37.65	37.49	0.164	0.474
Mother age	33.61	33.64	-0.0296	-0.100
Mother weight (kg)	56.71	55.81	0.897	1.803
Mother height (cm)	148.2	147.7	0.483*	1.989
Mother lang test (log)	4.288	4.254	0.0344	1.768

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The outcomes (Y_{ij}) included in the analysis[24] are the anthropometric, cognitive, motor skill, health, behavioral, and cash transfer variables described in *Section 2.3*. The controls (X_{ij}) considered through the analysis are:[25] (i) individual characteristics, such as sex, age, and number of siblings; (ii) baseline household characteristics, such as land ownership (ha), access to water and electricity, ownership of draft and small animals, and an asset ownership index;[26] (iii) parents' baseline (1997) characteristics, such as household head knowledge of indigenous language, father's years of schooling, father living in the household, as well as mother's age, height and score in a language test; and (iv) household demographic structure, including household size and proportion of individuals at different ages. *Table 2.1* includes descriptive statistics for these variables.

A second estimation considers the difference in the program's start between the original *treatment* (April 1998) and *control* (November 1999) localities and each child's date of birth. Five groups

[24] To avoid outliers, the estimations exclude outcomes below and above the percentiles 1 and 99, respectively.

[25] Missing controls are substituted in the analysis with the locality level means.

[26] The asset ownership index results from a principal component analysis that weights household's ownership of assets, including blender, refrigerator, gas stove, heater, radio, stereo, TV, video-player, washer, fan, car, and van.

based on the date of birth are formed and the following estimation is calculated:

$$Y_{ij} = \left(\sum_{k=1}^{5} \phi_k G.k * Treat_j \right) + \beta X_{ij} + \nu_j + \epsilon_{ij} \tag{2.2}$$

where the five parameters (ϕ_1-ϕ_5) show how exposure to the program at different stages of development might have influenced the medium-term outcomes analyzed. The groups (G.1-G.5) are formed as follows:

G.1: Born between July 1997-April 1998. Children born in *treatment* localities had additional exposure to the program during their early childhood

G.2: Born between May 1998-December 1998. Children born in *treatment* localities benefited partially while in-utero and during their early childhood

G.3: Born between January 1999-October 1999. Children born in *treatment* localities benefited during all its time in-utero and partially during early childhood

G.4: Born between November 1999-June 2000. Children born in *treatment* localities were benefited its complete time in-utero and those in *control* localities only partially

G.5: Born between July 2000-November 2001. Children born in *treatment* localities and *control* localities are benefited its complete time in-utero. But families in *treatment* localities had received the benefits for longer time

Previous work in the literature has argued that no effects result in the medium-term when comparing the original *treatment* and *control* localities because those children in the latter group catch-up with those in the former since both benefit from the program by November 1999. To test this argument, a regression discontinuity estimation compares the average outcomes of children just before and after the poverty index cutoff that determines eligibility. The rules of *Progresa* establish that once households in a locality are added to the program, new household additions or removals will not be considered until three years after the initial assessment. By limiting the sample to children in *treatment* localities, the discontinuity in enrollment at the poverty index threshold should stay constant for three years. Therefore, comparing the outcomes of children before and after the cutoff should give the difference of receiving the program from the start (April

1998) rather than three years later (April 2001), making the catch-up hypothesis less likely.

Finally, a last exercise attempts to isolate the effect of the cash component on the medium-term outcomes of children. *Progresa* rules indicate that no cash transfers are given for attendance until 3rd grade. *Table 2.3* shows the educational cash transfers that a household should receive for each child regularly attending school by child's date of birth[27] and semester. Two groups are formed: (i) Group I includes children that were born one semester after their oldest sibling's age is such that he/she should be attending school between 3rd and 5th grade; and (ii) Group II includes children that were born one semester after their oldest sibling's age is such that he should be attending school between *preprimaria*[28] and 2nd grade. For example, a child born on February 1999 would be in Group I if his oldest sibling was born between Sept. 2nd, 1988 and Sept. 1st, 1991.

The following specification is estimated:

$$Y_{ij} = \psi Cash\_Disc_{ij} + \beta X_{ij} + \nu_j + \epsilon_{ij} \tag{2.3}$$

where $Cash\_Disc_{ij}$ is equal to *one* (*zero*) if child i in locality j belongs to Group I (II).

Table 2.4 presents an exogeneity test showing that children in Group I and II are similar in terms of observable baseline characteristics. The only significant difference between the groups are the household size, number of siblings and parents' age. This is expected since Group I by construction has a slightly oldest first child. The estimations will show the difference in the results before and after controlling for these variables.

[27] Mexican regulations establish that a child should enroll to 1st grade the year in which he/she is six years old by September 1st. *Table 2.3* assumes that a child enrolls on time and continues his/her education without repeating any grade. Using date of birth (age) is preferred that using actual enrollment as in Manley et al. (2012) since considering enrollment to predict the transfers received could threat the exogeneity of the discontinuity.

[28] In Mexico, the last year of kindergarden is called *preprimaria* (pre-primary).

Table 2.3: Household's predicted monthly cash transfers per child, conditional on regular school attendance, by child's date of birth (values in Mexican Pesos).[a] Transfers assume that the child enrolls to school at the age specified by Mexican educational regulations.[b] Each column corresponds to a school year.[c]

Date of birth	1998-1999	1999-2000	2000-2001	2001-2002[d]	2002-2003
Sep 2, 1994 - Sep 1, 1995	-	-	-	-	-
Sep 2, 1993 - Sep 1, 1994	-	-	-	-	100
Sep 2, 1992 - Sep 1, 1993	-	-	-	95	115
Sep 2, 1991 - Sep 1, 1992	-	-	90	110	150
Sep 2, 1990 - Sep 1, 1991	-	80	105	145	200
Sep 2, 1989 - Sep 1, 1990	70	95	135	190	290
Sep 2, 1988 - Sep 1, 1989	80	125	180	280	310
Sep 2, 1987 - Sep 1, 1988	105	165	260	295	325
Sep 2, 1986 - Sep 1, 1987	135	240	275	310	-
Sep 2, 1985 - Sep 1, 1986	200	250	290	-	-
Sep 2, 1984 - Sep 1, 1985	210	265	-	-	-
Sep 2, 1983 - Sep 1, 1984	225	-	-	-	-

Amounts in Mexican pesos. The exchange rate during this time frame was on average 9.55 Mexican Pesos per U.S. dollar (Banco de México, 2012b).

[a] Amounts presented correspond to male transfers at the beginning of the school year. Cash transfers begin to be received when the child enrolls to 3rd grade and run until 9th grade. Beginning on 7th grade, transfers are higher for female (on average 6%, 11% and 15% for 7th, 8th and 9th grade correspondingly). For the second semester of the school year, transfers are adjusted (on average 5%). Additionally to educational transfers, each family receives a lump-sum transfer conditional on health attendance. Total household cash transfers are caped. This educational transfers assume that the cap has not been reached.

[b] Mexican regulations between 1997 and 2003 specified that children had to enroll on 1st grade on a given year if they are 6 years old by September 1st.

[c] A school year runs from mid-August to mid-June of the next year.

[d] Beginning on this year, transfers were given also for high school attendance (10th-12th grade). This amount are not presented in this table.

Table 2.4: Exogeneity tests for cash transfer discontinuity using baseline (1997) data[a]

Variable	Mean Group I	Mean Group II	Difference	t-statistic
Home characteristics				
Home owned	0.902	0.929	-0.0270	-0.955
Land owned	0.824	0.827	-0.00270	-0.0698
Dirt floor	0.725	0.760	-0.0348	-0.784
Water access	0.337	0.286	0.0511	1.087
Electricity access	0.637	0.719	-0.0821	-1.736
Asset ownership				
Blender	0.140	0.122	0.0174	0.509
Refrigerator	0.0415	0.0204	0.0210	1.199
Gas stove	0.114	0.153	-0.0391	-1.131
Heater	0.00518	0.00510	0.0000793	0.0109
Radio	0.487	0.469	0.0177	0.348
Stereo	0.0155	0.0459	-0.0304	-1.735
TV	0.254	0.296	-0.0420	-0.927
Video player	0.00518	0	0.00518	1.008
Washer	0.00518	0.0102	-0.00502	-0.565
Fan	0.0155	0.0357	-0.0202	-1.256
Van	0.0155	0.0153	0.000238	0.0190
Draft animals	0.259	0.296	-0.0369	-0.810
Other animals	0.767	0.750	0.0168	0.387
Family characteristics				
Poverty index	654.3	636.6	17.79*	2.431
Household size	4.560	5.510	-0.951***	-8.162
Num siblings	3.202	4.066	-0.864***	-6.748
Monthly income (MxP)	617.6	624.8	-7.265	-0.131
Land ownership (ha)	1.249	0.985	0.264	1.465
Parents characteristics				
Mother spk indig	0.461	0.464	-0.00315	-0.0621
Mother spk spanish	0.943	0.939	0.00423	0.176
Father spk indig	0.508	0.500	0.00777	0.153
Father spk spanish	0.979	0.974	0.00478	0.313
Father years educ	4.083	3.796	0.287	1.037
Mother years educ	4.114	3.735	0.379	1.424
Father age	34.21	36.08	-1.874***	-3.506
Mother age	29.70	32.56	-2.851***	-7.755

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Continued on next page

Table 2.4 – continued

Variable	Mean Group I	Mean Group II	Difference	t-statistic
Mother weight (kg)	55.15	55.81	-0.653	-0.652
Mother height (cm)	148.6	147.8	0.721	1.335
Mother lang test (log)	4.273	4.231	0.0425	1.021

[a] The discontinuity is identified using the age of the oldest sibling in the household and the educational cash transfers structure described in *Table 2.3*.
See *Section 2.4* for details.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

2.5 Results

This section presents the main results of the *Progresa* estimated impacts on early child development.

2.5.1 Effects of being born in an early versus late treatment locality

Table 2.5 shows that children born in *treatment* localities are exposed to additional cash transfers at their household level. On average, they receive $484, $530, and $1,959 Mexican pesos more during pregnancy, the first year, and cumulatively than households in *control* localities. Given *Progresa's* conditionality components, it would be expected that they also receive advantages from health care and nutrition while in-utero and/or early childhood.

Tables 2.6 and *2.7* present the results of the estimations described in equation 2.1. Each line corresponds to a different regression and each column to estimations using a different set of controls. They report the average difference of children's outcomes (ϕ) if they were born in a *treatment* (*early treatment*) instead of a *control* (*late treatment*) locality. *Table 2.6* includes the results for the anthropometric and cognitive outcomes and *Table 2.7* for the motor skills, health and behavioral outcomes. No significant difference between being born in a *treatment* rather than a *control* locality is found for any of these outcomes.

Table 2.5: First Stage effect of early versus late treatment on cash transfers received at the household level[a]

Dependent variable	Treatment[b] Model (1)	Treatment Model (2)	Treatment Model (3)	Treatment Model (4)	Treatment Model (5)
CCT pregnancy (MxP ,000)[c]	0.4804***	0.4822***	0.5050***	0.4675***	0.4835***
	(0.0578)	(0.0577)	(0.0566)	(0.0546)	(0.0557)
CCT 1st year (MxP ,000)[d]	0.5143***	0.5296***	0.5577***	0.5084***	0.5298***
	(0.0976)	(0.0947)	(0.0902)	(0.0918)	(0.0881)
CCT total (MxP ,000)[e]	1.6024**	1.7965***	2.0984***	1.7256***	1.9586***
	(0.6951)	(0.6771)	(0.6334)	(0.6415)	(0.6070)
Controls[h]					
Individual charact	✓	✓	✓	✓	✓
Baseline charact		✓	✓	✓	✓
Household demographics			✓		✓
Parents' charact				✓	✓

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered by village in parenthesis

[a] Each line corresponds to a different regression. Number of observations range between 2,082 and 2,251.

[b] *Treatment* coefficient represents the difference between receiving *early* versus *late* access to the program.

[c] Cash transfer amounts received during the 10 months previous to each child's date of birth. Values in thousand Mexican Pesos deflated to January 1998 values.

[d] Cash transfer amounts received during the 12 months after each child's date of birth. Values in thousand Mexican Pesos deflated to January 1998 values.

[e] Accumulated cash transfers received at the household level from the moment the household was added to the *Progresa* program up to June 2003. Values in thousand Mexican Pesos deflated to January 1998 values.

Table 2.6: Medium-term effect of early versus late treatment on anthropometric and cognitive development of children aged 2-6 years old[a]

Dependent variable	Treatment[b] Model (1)	Treatment Model (2)	Treatment Model (3)	Treatment Model (4)	Treatment Model (5)
Anthropometric					
Height (Z)[c]	-0.0642	-0.0439	-0.0388	-0.0284	-0.0279
	(0.1009)	(0.0991)	(0.0958)	(0.0865)	(0.0853)
Stunting (binary)[d]	0.0297	0.0225	0.0200	0.0220	0.0226
	(0.0488)	(0.0484)	(0.0469)	(0.0427)	(0.0419)
Weight (Z)[c]	-0.0212	-0.0162	-0.0168	-0.0022	-0.0077
	(0.0608)	(0.0573)	(0.0561)	(0.0492)	(0.0489)
BMI (Z)[c]	0.0390	0.0238	0.0196	0.0357	0.0318
	(0.0685)	(0.0666)	(0.0655)	(0.0624)	(0.0616)
Overweight (binary)[e]	-0.0075	-0.0115	-0.0140	-0.0092	-0.0108
	(0.0219)	(0.0221)	(0.0220)	(0.0210)	(0.0211)
Cognitive tests					
LT memory (log)[f]	0.0160	0.0321	0.0352	0.0502	0.0554
	(0.0535)	(0.0467)	(0.0463)	(0.0429)	(0.0423)
ST memory (log)[f]	0.0086	0.0073	0.0072	0.0100	0.0096
	(0.0361)	(0.0329)	(0.0326)	(0.0259)	(0.0259)
Visual-spatial (log)[f]	-0.0353	-0.0269	-0.0279	-0.0139	-0.0139
	(0.0398)	(0.0379)	(0.0383)	(0.0357)	(0.0364)
Language (log)[g]	0.0163	0.0189	0.0223	0.0271	0.0301
	(0.0636)	(0.0585)	(0.0574)	(0.0535)	(0.0526)
Controls[h]					
Individual charact	✓	✓	✓	✓	✓
Baseline charact		✓	✓	✓	✓
Household demographics			✓		✓
Parents' charact				✓	✓

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered by village in parenthesis

[a] Each line corresponds to a different regression. Number of observations range between 1,477 and 2,120.

[b] *Treatment* coefficient represents the difference between receiving *early* versus *late* access to the program.

[c] Weight, height and BMI are standardized with respect to a same age-sex healthy reference population following WHO guidelines.

[d] Stunting is a binary variable equal to one if an individual's height corresponds to being two or more standard deviations below the same age-sex standardized height of a healthy reference population.

[e] Overweight is a binary variable equal to one if an individual's BMI corresponds to being above the 85th percentile of a same age-sex standardized BMI of a health reference population.

[f] Long and short term memory and visual spatial integration are assessed using the Woodcock-Muñoz Test in children aged 2-6.

[g] Language development is measured using the Peabody test in children aged 3-6.

[h] See *Table 2.1* for details of variables included as controls.

Table 2.7: Medium-term effect of early versus late treatment on motor skills, health, and behavioral development of children aged 2-6 years old[a]

Dependent variable	Treatment[b] Model (1)	Treatment Model (2)	Treatment Model (3)	Treatment Model (4)	Treatment Model (5)
Motor skills					
Balance (secs)[c]	-0.0565	-0.0287	-0.0243	0.0738	0.0837
	(0.2208)	(0.2182)	(0.2227)	(0.2247)	(0.2307)
Walk back (binary)[c]	0.0335*	0.0295	0.0302	0.0209	0.0217
	(0.0195)	(0.0189)	(0.0192)	(0.0193)	(0.0197)
Tiptoe (binary)[c]	0.0173	0.0144	0.0146	0.0239	0.0236
	(0.0214)	(0.0211)	(0.0214)	(0.0224)	(0.0229)
Walk straight (binary)[c]	-0.0050	-0.0063	-0.0061	-0.0109	-0.0109
	(0.0174)	(0.0169)	(0.0170)	(0.0173)	(0.0177)
Jump (binary)[c]	0.0053	0.0072	0.0091	0.0006	0.0014
	(0.0271)	(0.0252)	(0.0245)	(0.0275)	(0.0267)
Health and behavioral					
Hemoglobin (g/dL)[d]	0.0231	0.0365	0.0387	0.0397	0.0469
	(0.0754)	(0.0765)	(0.0769)	(0.0765)	(0.0762)
Days sick[e]	-0.0659	-0.0925	-0.0995	-0.0688	-0.0783
	(0.1392)	(0.1374)	(0.1369)	(0.1443)	(0.1427)
Depression (Z-score)[f]	0.0348	0.0238	0.0282	0.0142	0.0171
	(0.0588)	(0.0544)	(0.0547)	(0.0546)	(0.0547)
Aggression (Z-score)[f]	-0.0197	-0.0307	-0.0282	-0.0431	-0.0409
	(0.0585)	(0.0591)	(0.0597)	(0.0614)	(0.0625)
Controls[h]					
Individual charact	✓	✓	✓	✓	✓
Baseline charact		✓	✓	✓	✓
Household demographics			✓		✓
Parents' charact				✓	✓

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered by village in parenthesis

[a] Each line corresponds to a different regression. Number of observations range between 1,805 and 2,254.

[b] *Treatment* coefficient represents the difference between receiving *early* versus *late* access to the program.

[c] McCarthy Scale of Children's Abilities is used to assess motor skills on children aged 2-6.

[d] Hemoglobin concentration was adjusted by village's altitude following WHO standards.

[e] Children's mother self reports the number of days that the child has been sick during the past 4 weeks.

[f] Depression and aggression are Z scores of an index calculated using behavioral questions answered by the child's mother. The procedure to calculate the index follows Achenbach and Rescorla (2000) CBCL.

[g] Total cash transfer amounts received by the household from the moment they signed up to the program until June 2003. Cash transfers deflated using Mexico's CPI (Banco de México, 2012a).

[h] See Table 2.1 for details of variables included as controls.

2.5.2 Heterogenous effects by stages of development

Given the difference of date in which the program started in the *treatment* (April 1998) and *control* (November 1999) localities, it would be expected that the program has heterogenous effects on children depending on their dates of birth. As described in *Section 2.4*, five different groups are formed based on dates of birth to analyze this heterogeneity. *Table 2.8* shows the advantage of being born in a *treatment* locality in terms of the cash transfers received by birthdate group. Cash transfers received during pregnancy are $413, $1,446, and $985 Mexican pesos significantly higher for households in groups G.2, G.3, and G.4 (i.e. children born May 98-Jun 00) inhabiting in a *treatment* locality, respectively. Similarly, cash transfers received during the first year of life are $1,114, $1,789, and $758 Mexican pesos significantly higher for households in groups G.1, G.2, and G.3 (i.e. children born Jul 97-Oct 99) living in a *treatment* locality, respectively. Cumulative transfers are also significantly higher for households with children at all groups that were born in a *treatment* locality.

Tables 2.9, *2.10* and *2.11* show the results of anthropometric, cognitive and motor skills' development. No significant results are found for *Progresa* exposure during vital stages of early child development. The only consistent evidence is found for group G.2 (born May 98-Dec 98) which corresponds to children that, for being born in a *treatment* locality, receive $413 and $1,789 Mexican pesos more during pregnancy and the first year of life as well as better exposure to health services during their in-utero and early childhood development. This group consistently exhibits positive effects of the exposure to *Progresa* on the anthropometric, cognitive, and motor skill development outcomes, but just two motor skill development indicators are significant at a 10% level. Health and behavioral outcomes were also analyzed and no significant effects were found for any of the groups.[29]

2.5.3 Test for children in the late treatment group catching-up

Most of the results found in *Tables 2.6* to *2.11* show no significant advantages of children from the original *treatment* localities (i.e. the *early treatment*). These results are consistent with previous

[29] Results available upon request.

Table 2.8: First stage: Cash transfers received at the household level. Divided by timing in which the treatment began to be received with respect to child's date of birth

	CCT preg (MxP ,000)[a] (1)	CCT 1st yr (MxP ,000)[b] (2)	CCT total (MxP ,000)[c] (3)
Treatment[d] x G.1[e]	0.0378	1.114***	3.836***
	(0.0418)	(0.1042)	(0.7606)
Treatment x G.2[f]	0.413***	1.789***	2.558***
	(0.0537)	(0.1244)	(0.7788)
Treatment x G.3[g]	1.446***	0.758***	1.922***
	(0.0756)	(0.1290)	(0.6400)
Treatment x G.4[h]	0.985***	0.177	2.772***
	(0.0893)	(0.1476)	(0.6149)
Treatment x G.5[i]	0.0617	-0.280	2.684***
	(0.1219)	(0.1883)	(0.6530)
Observations	1870	1624	1805
R^2	0.61	0.54	0.47

Controlling for household demographics, individual, baseline and parents' characteristics

Standard errors clustered by village (in parenthesis)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[a] Cash transfer amounts received during the 10 months previous to each child's date of birth. Values in thousand Mexican Pesos deflated to January 1998 values.

[b] Cash transfer amounts received during the 12 months after each child's date of birth. Values in thousand Mexican Pesos deflated to January 1998 values.

[c] Accumulated cash transfers received at the household level from the moment the household was added to the *Progresa* program up to June 2003. Values in thousand Mexican Pesos deflated to January 1998 values.

[d] Treatment villages begin to receive transfers in April 1998 and control villages in November 1999.

[e] G.1: Born Jul 97 - Apr 98. If on a Treatment locality, these children were benefited during its early childhood (beginning ages 0-10 months), but not during pregnancy.

[f] G.2: Born May 98 - Dec 98. If on a Treatment locality, these children were benefited during part of the pregnancy and in early childhood.

[g] G.3: Born Jan 99 - Oct 99. If on a Treatment locality, these children were benefited during all of the pregnancy and in early childhood.

[h] G.4: Born Nov 99 - Jun 00. If on a Treatment locality, these children were benefited during all of the pregnancy while those in control localities were benefited in part of the pregnancy.

[i] G.5: Born Jul 00 - Nov 01. Both children on treatment and control localities were benefited during pregnancy. Children's families on treatment localities have been received benefits for longer.

Table 2.9: Medium-term effects of Treatment on children's anthropometric development. Effects classified by timing in which the treatment began to be received with respect to child's date of birth[a]

	Height (Z)[b] (1)	Stunt[c] (2)	Weight (Z)[b] (3)	BMI (Z)[b] (4)	Overweight[d] (5)
Treatment[e] x G.2[f]	0.0448	-0.0247	0.0939	0.0811	-0.0435
	(0.1237)	(0.0718)	(0.1030)	(0.1104)	(0.0362)
Treatment x G.3[g]	-0.0351	0.0388	0.0439	0.0538	0.00173
	(0.1057)	(0.0521)	(0.0766)	(0.0784)	(0.0309)
Treatment x G.4[h]	-0.132	0.0371	-0.143	0.0168	0.00833
	(0.1163)	(0.0637)	(0.0885)	(0.0896)	(0.0363)
Treatment x G.5[i]	0.00509	0.0169	-0.00179	0.00551	-0.0234
	(0.1213)	(0.0527)	(0.0766)	(0.1062)	(0.0403)
Observations	1479	1507	1493	1481	1506
R^2	0.25	0.16	0.14	0.09	0.07

Controlling for household demographics, individual, baseline and parents' characteristics

Standard errors clustered by village (in parenthesis)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[a] No observations for G.1 available since WHO software standardization not available for those ages.

[b] Weight, height and BMI are standardized with respect to a same age-sex healthy reference population following WHO guidelines.

[c] Stunting is a binary variable equal to one if an individual's height corresponds to being two or more standard deviations below the same age-sex standardized height of a healthy reference population (World Health Organization, 2012).

[d] Overweight is a binary variable equal to one if an individual's BMI corresponds to being above the 85 percentile of a same age-sex standardized BMI of a health reference population (World Health Organization, 2012).

[e] Treatment villages begin to receive transfers in April 1998 and control villages in November 1999.

[f] G.2: Born May 98 - Dec 98. If on a Treatment locality, these children were benefited during part of the pregnancy and in early childhood.

[g] G.3: Born Jan 99 - Oct 99. If on a Treatment locality, these children were benefited during all of the pregnancy and in early childhood.

[h] G.4: Born Nov 99 - Jun 00. If on a Treatment locality, these children were benefited during all of the pregnancy while those in control localities were benefited in part of the pregnancy.

[i] G.5: Born Jul 00 - Nov 01. Both children on treatment and control localities were benefited during pregnancy. Children's families on treatment localities have been received benefits for longer.

Table 2.10: Medium-term effects of Treatment on children's cognitive development measured with the *Peabody Picture Vocabulary Test* and *Batería Woodcock-Muñoz Test*. Effects divided by timing in which the treatment began to be received with respect to child's date of birth

	Peabody Test[a]	**Woodcock-Muñoz Test**[b]		
	Language	LT memory	ST memory	Visual-spatial
	(1)	(2)	(3)	(4)
Treatment[c] x G.1[d]	0.00499	0.103	0.0142	0.0412
	(0.0802)	(0.0829)	(0.0339)	(0.0385)
Treatment x G.2[e]	0.0632	0.121	0.0340	0.0209
	(0.0974)	(0.0955)	(0.0444)	(0.0581)
Treatment x G.3[f]	0.101	0.145*	0.0726	0.0123
	(0.0807)	(0.0741)	(0.0459)	(0.0505)
Treatment x G.4[g]	-0.0373	-0.0124	-0.114*	0.00553
	(0.0863)	(0.0698)	(0.0650)	(0.0803)
Treatment x G.5[h]	-0.0806	-0.0794	0.0274	-0.197**
	(0.1699)	(0.0679)	(0.0578)	(0.0850)
Observations	1584	1962	1866	1627
R^2	0.35	0.34	0.46	0.37

Controlling for household demographics, individual, baseline and parents' characteristics

Standard errors clustered by village (in parenthesis)

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[a] Peabody test measures language development in children aged 3-6. Peabody tests have been shown to be a reliable predictor of achievement in primary school(Duncan et al., 2007).

[b] Woodcock-Muñoz Test measures different cognitive abilities in children 2-6. The ENCEL 2003 dataset contains test scores from subtests that measure long-term memory, short-term memory and visual-spatial integration.

[c] Treatment villages begin to receive transfers in April 1998 and control villages in November 1999.

[d] G.1: Born Jul 97 - Apr 98. If on a Treatment locality, these children were benefited during its early childhood (beginning ages 0-10 months), but not during pregnancy.

[e] G.2: Born May 98 - Dec 98. If on a Treatment locality, these children were benefited during part of the pregnancy and in early childhood.

[f] G.3: Born Jan 99 - Oct 99. If on a Treatment locality, these children were benefited during all of the pregnancy and in early childhood.

[g] G.4: Born Nov 99 - Jun 00. If on a Treatment locality, these children were benefited during all of the pregnancy while those in control localities were benefited in part of the pregnancy.

[h] G.5: Born Jul 00 - Nov 01. Both children on treatment and control localities were benefited during pregnancy. Children's families on treatment localities have been received benefits for longer.

Table 2.11: Medium-term effects of Treatment on children's motor skills development measured with the *McCarthy Scale of Children's Abilities* (*MSCA*).[a] Effects divided by timing in which the treatment began to be received with respect to child's date of birth

	Balance (secs)	Walk back	Tiptoe	Walk straight	Jump coord
	(1)	(2)	(3)	(4)	(5)
Treatment[b] x G.1[c]	0.480	0.00424	0.0210	-0.0162	0.00634
	(0.4014)	(0.0211)	(0.0273)	(0.0218)	(0.0557)
Treatment x G.2[d]	0.791*	0.0296	0.0615*	0.00871	0.0288
	(0.4274)	(0.0315)	(0.0371)	(0.0350)	(0.0634)
Treatment x G.3[e]	-0.399	0.0171	-0.00515	-0.00156	-0.0371
	(0.3826)	(0.0283)	(0.0413)	(0.0385)	(0.0508)
Treatment x G.4[f]	-0.574	-0.0254	-0.000410	-0.0658	-0.0240
	(0.4486)	(0.0484)	(0.0516)	(0.0459)	(0.0388)
Treatment x G.5[g]	0.136	0.0768	0.0487	0.0168	0.0242
	(0.3789)	(0.0501)	(0.0520)	(0.0511)	(0.0221)
Observations	1880	2014	1960	2004	1809
R^2	0.34	0.16	0.28	0.22	0.22

Controlling for household demographics, individual, baseline and parents' characteristics
Standard errors clustered by village (in parenthesis)
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[a] McCarthy Scale of Children's Abilities is used to assess motor skills on children aged 2-6.
[b] Treatment villages begin to receive transfers in April 1998 and control villages in November 1999.
[c] G.1: Born Jul 97 - Apr 98. If on a Treatment locality, these children were benefited during its early childhood (beginning ages 0-10 months), but not during pregnancy.
[d] G.2: Born May 98 - Dec 98. If on a Treatment locality, these children were benefited during part of the pregnancy and in early childhood.
[e] G.3: Born Jan 99 - Oct 99. If on a Treatment locality, these children were benefited during all of the pregnancy and in early childhood.
[f] G.4: Born Nov 99 - Jun 00. If on a Treatment locality, these children were benefited during all of the pregnancy while those in control localities were benefited in part of the pregnancy.
[g] G.5: Born Jul 00 - Nov 01. Both children on treatment and control localities were benefited during pregnancy. Children's families on treatment localities have been received benefits for longer.

findings in the literature that argue that the no-effect results from a catch-up of the children on the late treatment localities (Neufeld et al., 2005). The catch-up hypothesis claims that both groups actually benefit from the program.

The regression discontinuity design estimated here attempts to shed some light on the catch-up hypothesis. It takes advantage from the fact that once localities are added to the program, a new reassessment to add or remove additional households does not happen until three years later. *Figure 2.1* shows the proportion of households that became beneficiaries of *Progresa* depending on their poverty index in the *treatment* localities. *Progresa's* rules establish that eligibility based on the poverty index should yield a sharp regression discontinuity for this group of households. The top left panel in *Figure 2.1* shows that the selection based on the poverty index was applied as expected at the beginning of the program (April 1998). The top right panel shows that the discontinuity remained until December 2000, although by then some additional households just below the eligibility threshold had already been added. Finally, the bottom left panel shows that a re-assessment was effectively done by April 2001 and additional households were added, breaking the original discontinuity. The reassessment was done based on a different model, which means that the original 1997 poverty index is no longer the reference to determine eligibility. Finally, the bottom right panel shows the picture at the moment the 2003 survey was collected.

Figures 2.2 to *2.4* show no evidence of a medium-term effect on a selected group of anthropometric, cognitive and motor skills outcomes for early exposure to the program. These results contrast with the catch-up hypothesis and are more consistent with the no-effect argument. The anthropometric, cognitive, motor skill, health and behavioral outcomes not presented were also analyzed. No effects were found in any of those cases either.[30]

2.5.4 Effects of the cash component

The results presented contrast with previous findings in the literature that indicate that increases in the *Progresa* cash component result in anthropometric, cognitive and motor skill im-

[30] Graphs available upon request

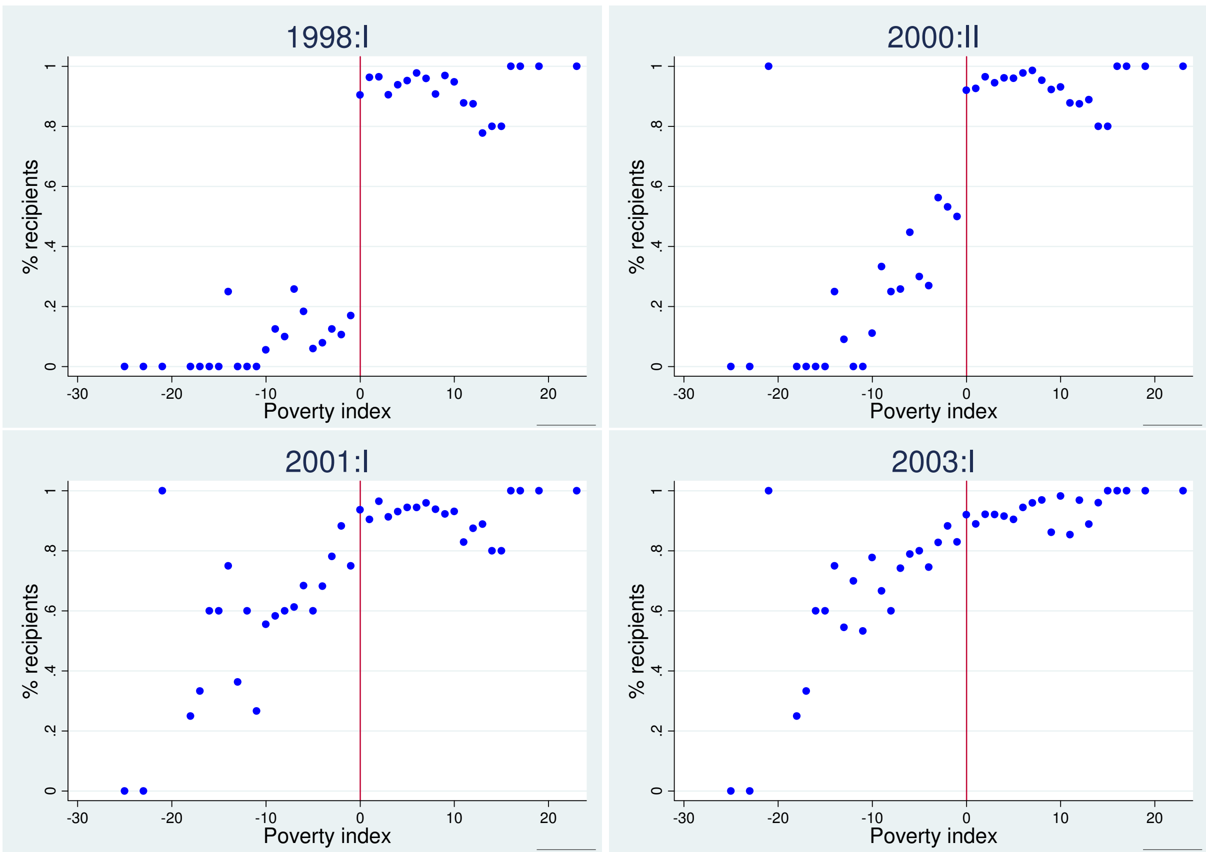


Figure 2.1: Regression Discontinuity: First Stage ID

On each graph, the x-axis corresponds to the poverty index used by the administrative rule to select *Progresa* beneficiaries. The administrative cutoff is centered at zero.
The poverty index is formed with a formula that weights household's asset ownership and socio-economic characteristics of its members.
Analysis restricted to original randomized treatment villages. These villages begin to receive the transfers on April 1998 and are reassessed three years later to consider including more households.
The y-axis gives the proportion of households that report receiving the cash transfers of the program. Perfect targeting and take-up rates would yield a sharp regression discontinuity.

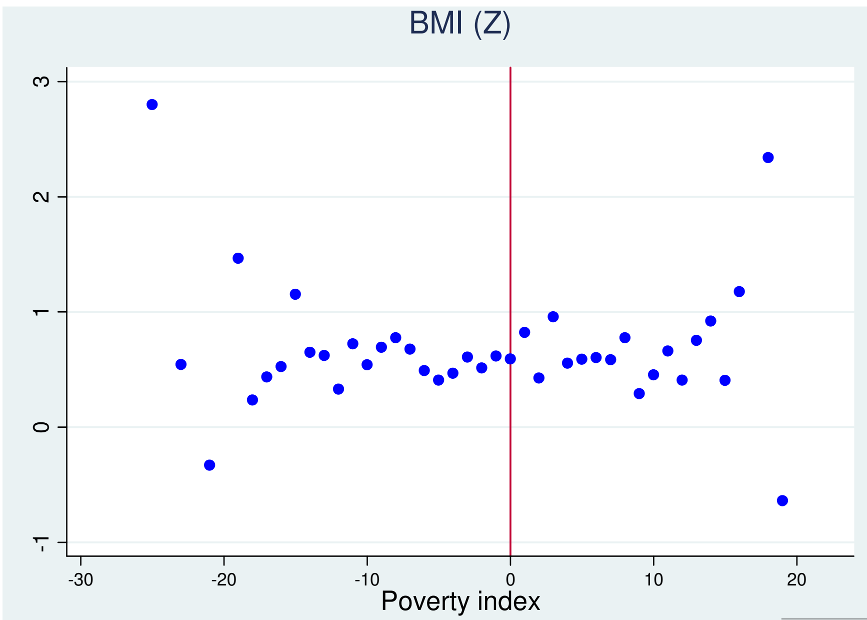


Figure 2.2: Regression discontinuity analysis: anthropometric outcomes

On each graph, the x-axis corresponds to the standardized poverty index used by the administrative rule to select *Progresa* beneficiaries. The administrative cutoff is centered at zero.
Analysis restricted to original randomized treatment villages.
The y-axis gives conditional means of the individual outcomes.

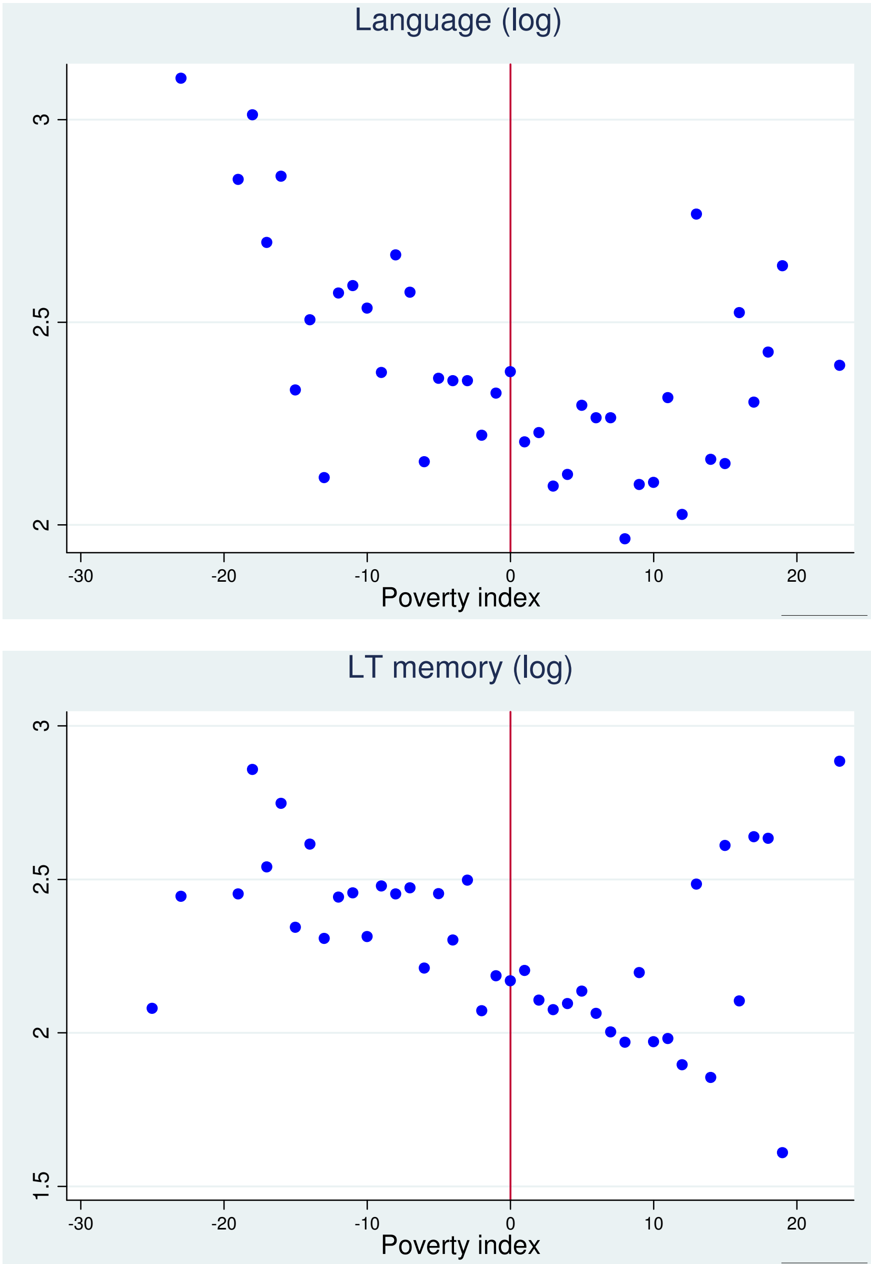
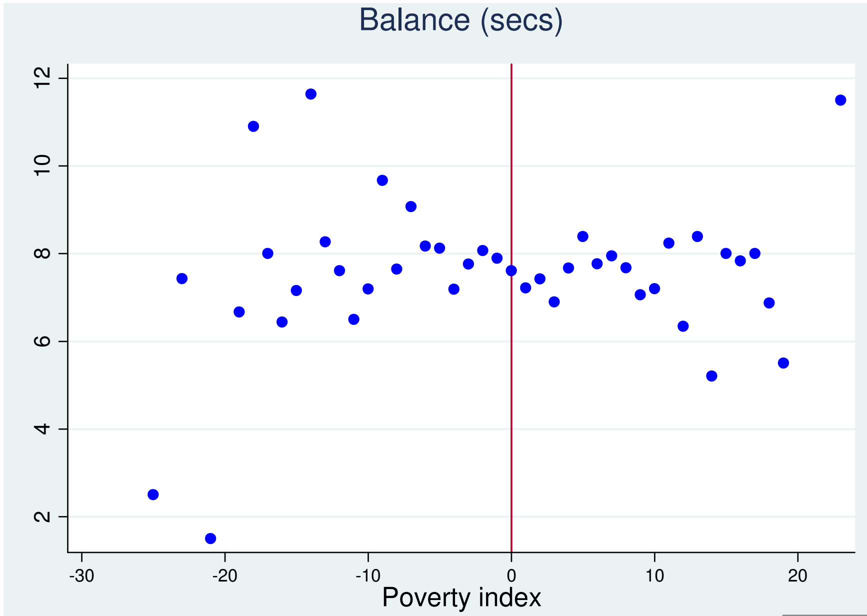


Figure 2.3: Regression discontinuity analysis: cognitive outcomes

On each graph, the x-axis corresponds to the standardized poverty index used by the administrative rule to select *Progresa* beneficiaries. The administrative cutoff is centered at zero.
Analysis restricted to original randomized treatment villages.
The y-axis gives conditional means of the individual outcomes.



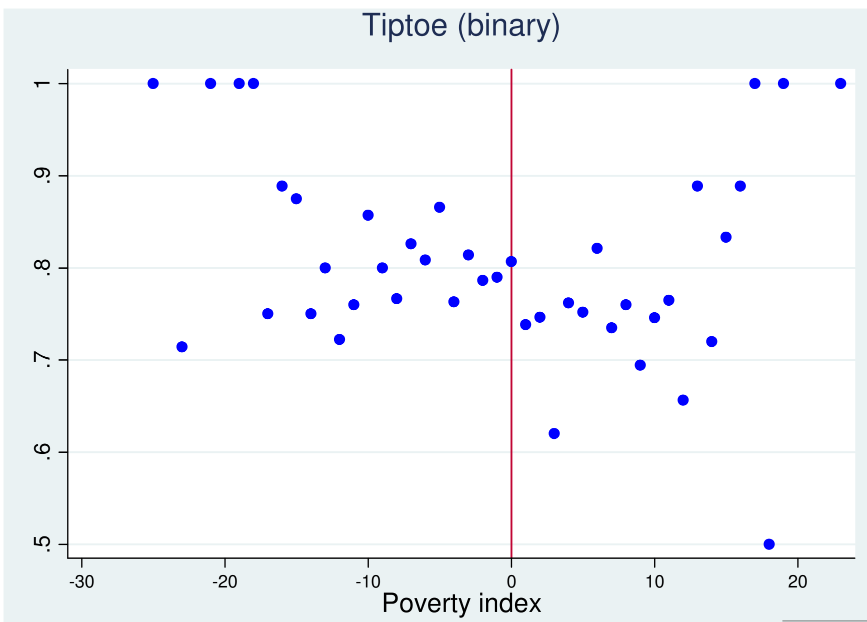


Figure 2.4: Regression discontinuity analysis: motor skills outcomes

On each graph, the x-axis corresponds to the standardized poverty index used by the administrative rule to select *Progresa* beneficiaries. The administrative cutoff is centered at zero.
Analysis restricted to original randomized treatment villages.
The y-axis gives conditional means of the individual outcomes.

provements (Fernald et al., 2008; Manley et al., 2012). The final specification, described in equation 2.3, estimates the result of discrete increases in cash transfers at the household level that result from the age of the oldest sibling and *Progresa* transfers' structure. *Table 2.12* shows that the identification design effectively reflects differences in the cash flows received at those children's households. Children in households that receive the cash benefit are related to estimated increases in cash transfers equal to \$158, \$344 and \$1,493 Mexican pesos during pregnancy, the first year of life, and total accumulated.

Tables 2.13 and *2.14* show the results for the anthropometric, cognitive, motor skill, health, and behavioral outcomes. Each line corresponds to a different regression and each column to estimations using a different set of controls. The effect of the cash discontinuity (ψ) is reported. *Table 2.13* shows some positive effects of the cash transfers on standardized height and long-term memory as well as decrements in the likelihood of stunting and overweight. However, the effects dilute when controls for household demographics are included. Finally, the evidence from *Table 2.14* finds no effects form the cash transfers on motor skill, health and behavioral outcomes.

A threat to the identification's validity could be that parents in the group that receives higher transfers might also comply in a higher proportion and more timely the conditionalities (since their cost of not doing so is higher). Also, the higher incentive of sending the oldest child to school might result in parents' higher awareness of the importance of child development. Finally, having older sibling whose school participation results in higher transfers to the family might divert parents' attention from their younger children.

Table 2.12: Relation between the cash discontinuity variable (*Cash_Disc*) and actual cash transfers[a]

Dependent variable	*Cash_Disc*[b] Model (1)	*Cash_Disc* Model (2)	*Cash_Disc* Model (3)	*Cash_Disc* Model (4)	*Cash_Disc* Model (5)
Cash flows					
CCT pregnancy (MxP ,000)[c]	0.4307***	0.4315***	0.1790**	0.4060***	0.1584**
	(0.0494)	(0.0473)	(0.0736)	(0.0489)	(0.0775)
CCT 1st year (MxP ,000)[d]	0.8452***	0.8390***	0.3974***	0.7732***	0.3435***
	(0.0742)	(0.0730)	(0.1336)	(0.0696)	(0.1309)
CCT total (MxP ,000)[e]	4.1472***	4.0794***	1.8780***	3.6734***	1.4927**
	(0.3904)	(0.3831)	(0.6973)	(0.3946)	(0.7386)
Controls[f]					
Individual charact	✓	✓	✓	✓	✓
Baseline charact		✓	✓	✓	✓
Household demographics			✓		✓
Parents' charact				✓	✓

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered by village in parenthesis

[a] Each line corresponds to a different regression. Number of observations range between 287 and 377.

[b] The discontinuity is identified using the age of the oldest sibling in the household and the educational cash transfers structure described in *Table 2.3*. See *Section 2.4* for details.

[c] Cash transfer amounts received during the 10 months previous to each child's date of birth. Values in thousand Mexican Pesos deflated to January 1998 values.

[d] Cash transfer amounts received during the 12 months after each child's date of birth. Values in thousand Mexican Pesos deflated to January 1998 values.

[e] Accumulated cash transfers received at the household level from the moment the household was added to the *Progresa* program up to June 2003. Values in thousand Mexican Pesos deflated to January 1998 values.

[f] See *Table 2.1* for details of variables included as controls.

Table 2.13: Medium-term effect of additional household cash transfers on anthropometric and cognitive development of children aged 2-6 years old.[a]

Dependent variable	*Cash_Disc*[b] Model (1)	*Cash_Disc* Model (2)	*Cash_Disc* Model (3)	*Cash_Disc* Model (4)	*Cash_Disc* Model (5)
Anthropometric					
Height (Z)[c]	0.2674**	0.2610**	0.0649	0.3050***	0.1539
	(0.1182)	(0.1170)	(0.1535)	(0.1107)	(0.1389)
Stunting (binary)[d]	-0.0921*	-0.0916*	-0.0106	-0.0723	-0.0199
	(0.0542)	(0.0536)	(0.0685)	(0.0520)	(0.0632)
Weight (Z)[c]	0.0970	0.0979	0.0164	0.1864**	0.1484
	(0.0804)	(0.0827)	(0.1104)	(0.0797)	(0.1076)
BMI (Z)[c]	-0.1438	-0.1406	-0.0702	-0.0522	0.0161
	(0.0901)	(0.0909)	(0.1202)	(0.0975)	(0.1210)
Overweight (binary)[e]	-0.0718**	-0.0686**	-0.0244	-0.0419	-0.0075
	(0.0346)	(0.0346)	(0.0559)	(0.0374)	(0.0537)
Cognitive tests					
LT memory (log)[f]	0.0036	0.0001	-0.0497	0.0461	0.0175
	(0.0961)	(0.0970)	(0.1282)	(0.0961)	(0.1305)
ST memory (log)[f]	0.1028*	0.1042*	0.0395	0.1072	0.0364
	(0.0588)	(0.0610)	(0.0838)	(0.0658)	(0.0799)
Visual-spatial (log)[f]	0.0304	0.0275	0.0597	0.0560	0.1305
	(0.0683)	(0.0682)	(0.1062)	(0.0682)	(0.0994)
Language (log)[g]	-0.0585	-0.0673	-0.0836	-0.0597	-0.0474
	(0.0854)	(0.0861)	(0.1028)	(0.0995)	(0.1036)
Controls[h]					
Individual charact	✓	✓	✓	✓	✓
Baseline charact		✓	✓	✓	✓
Household demographics			✓		✓
Parents' charact				✓	✓

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered by village in parenthesis

[a] Each line corresponds to a different regression. Number of observations range between 247 and 424.

[b] The discontinuity is identified using the age of the oldest sibling in the household and the educational cash transfers structure described in *Table 2.3*. See *Section 2.4* for details.

[c] Weight, height and BMI are standardized with respect to a same age-sex healthy reference population following WHO guidelines.

[d] Stunting is a binary variable equal to one if an individual's height corresponds to being two or more standard deviations below the same age-sex standardized height of a healthy reference population.

[e] Overweight is a binary variable equal to one if an individual's BMI corresponds to being above the 85 percentile of a same age-sex standardized BMI of a health reference population.

[f] Long and short term memory and visual spatial integration are assessed using the Woodcock-Muñoz Test in children aged 2-6.

[g] Language development is measured using the Peabody test in children aged 3-6.

[h] See *Table 2.1* for details of variables included as controls.

Table 2.14: Medium-term effect of additional household cash transfers on motor skills, health, and behavioral development of children aged 2-6 years old[a]

Dependent variable	*Cash_Disc*[b] Model (1)	*Cash_Disc* Model (2)	*Cash_Disc* Model (3)	*Cash_Disc* Model (4)	*Cash_Disc* Model (5)
Motor skills					
Balance (secs)[c]	-0.0149	0.0187	-0.2083	0.0229	-0.1853
	(0.3928)	(0.3938)	(0.6667)	(0.4272)	(0.6857)
Walk back (binary)[c]	-0.0507	-0.0447	-0.0193	-0.0362	-0.0163
	(0.0450)	(0.0453)	(0.0687)	(0.0472)	(0.0704)
Tiptoe (binary)[c]	-0.0282	-0.0180	0.0035	-0.0208	0.0013
	(0.0484)	(0.0481)	(0.0739)	(0.0510)	(0.0759)
Walk straight (binary)[c]	-0.0697	-0.0617	-0.0684	-0.0676	-0.0667
	(0.0486)	(0.0489)	(0.0615)	(0.0508)	(0.0612)
Jump (binary)[c]	0.0090	0.0139	0.0042	-0.0003	-0.0003
	(0.0339)	(0.0329)	(0.0470)	(0.0351)	(0.0489)
Health and behavioral					
Hemoglobin (g/dL)[d]	-0.0323	-0.0555	-0.0229	0.0233	0.0267
	(0.1660)	(0.1645)	(0.2014)	(0.1704)	(0.2144)
Days sick[e]	-0.0506	-0.0121	-0.0679	-0.1395	-0.1868
	(0.2788)	(0.2953)	(0.3742)	(0.3675)	(0.4287)
Depression (Z-score)[f]	-0.0363	-0.0208	0.1527	0.0087	0.1394
	(0.1023)	(0.0998)	(0.1194)	(0.1098)	(0.1221)
Aggression (Z-score)[f]	-0.1884*	-0.1781*	-0.1036	-0.1510	-0.0899
	(0.1039)	(0.1066)	(0.1449)	(0.1243)	(0.1560)
Controls[g]					
Individual charact	✓	✓	✓	✓	✓
Baseline charact		✓	✓	✓	✓
Household demographics			✓		✓
Parents' charact				✓	✓

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Standard errors clustered by village in parenthesis

[a] Each line corresponds to a different regression. Number of observations range between 333 and 417.

[b] The discontinuity is identified using the age of the oldest sibling in the household and the educational cash transfers structure described in *Table 2.3*. See *Section 2.4* for details.

[c] McCarthy Scale of Children's Abilities is used to assess motor skills on children aged 2-6.

[d] Hemoglobin concentration was adjusted by village's altitude following WHO standards.

[e] Children's mother self reports the number of days that the child has been sick during the past 4 weeks.

[f] Depression and aggression are Z scores of an index calculated using behavioral questions answered by the child's mother. The procedure to calculate the index follows Achenbach and Rescorla (2000) CBCL.

[g] See *Table 2.1* for details of variables included as controls.

2.6 Conclusions

Conditional cash transfer programs have become widely popular in developing countries. Particularly in Latin America, the number of countries implementing CCTs went from 2 at the end of the 1990's (Mexico and Brazil) to 17 by 2008. Most of these programs' motivation is to improve human capital acquisition among the poor in order to alleviate the disadvantages of children born in these settings. A recent growing literature has underlined the importance of early child development and has shown that deficiencies during early childhood tend to have a long-term influence on individuals' lives. A vast body of work has analyzed the impacts that CCT programs have on several dimensions of peoples' lives. However, little attention has been paid to investigate whether children are in adequate development conditions (physical, cognitive, health and behavioral) before entering school. If children are already disadvantaged, then it is likely that they will not be able to benefit as much from their added human capital investments. This argument should be of great concern from a policy point of view and efficient use of resources.

This paper benefits from a rich dataset that was gathered as part of *Progresa's* follow-up surveys. The information includes objective indicators of anthropometric, cognitive, motor skills, health, and behavioral development of preschool children from the original experimental localities. Even though, the design of *Progresa* includes components intended to benefit children at their early development stages, no significant effects on medium-term development were found. As described in *Section 2.3*, these children are, on average, 1.85 standard deviations below a healthy reference population height and between the 7 and 21 percentile of cognitive test with respect to a Latin-American reference population. This serious lag in physical and cognitive development, combined with the lack of CCT benefits found in this paper raise an important concern.

The evidence presented in this paper is based on the original *Progresa* experiment localities, which are representative of rural and marginalized communities in Mexico. The results and analyses might differ for localities that were later added to the program, particularly those in urban settings. However, given that one of *Progresa's* main goals is to close the inequality gap for future generations, attention should be paid to the results presented here.

3. EDUCATIONAL SELF-SELECTION AMONG U.S. IMMIGRANTS AND RETURNING MIGRANTS

3.1 Introduction

International migration is a topic of great interest in multiple fields, like demography, politics, law, sociology, and economics. During the last decades, the number of international migrants has risen to a great extent. On 2010, it is estimated that roughly 214 million people migrated, being the United States the main destination. The most recent figures indicate that the U.S. comprises more than 40 million foreign-born inhabitants, which account for more than 12% of its population (Koser and Laczko, 2010).

This paper empirically analyses from a historical perspective the selectivity that immigrants and returning migrants exhibit in terms of schooling. The United States is considered to be the host country and a group of ten sending countries are selected based on their historical contribution towards migration to the U.S. The purpose is to describe, on a country-by-country basis, the selectivity of incoming migrants with respect to their home country's schooling distribution. Then, synthetic cohorts of immigrants with similar characteristics are followed through census years to assess the kind of selectivity that results from returning migration.

From a microeconomic point of view, migration has been studied as a rational choice made by maximizing agents. The literature began by identifying migrants as a group of individuals that share some characteristics, like being more ambitious, highly motivated, and hard-working (Carliner, 1980; Chiswick, 1978). Some years later, in one of the most influential theoretical papers written on the topic, Borjas (1987, 1991) developed an application of Roy's model (Roy, 1951) to explain how migrants self-select from the source countrys income distribution. According to that model, immigrants arriving from a country that has a higher (lower) level of inequality[1] than the host country, would negatively (positively) select from the source country's income distribution.

In a later paper, Borjas and Bratsberg (1996) extended the Borjas (1987, 1991) model to account for migrants returning to their country of origin. They concluded that returning migration accentuates the kind of selection that resulted from immigration. This conclusion is relevant from a political point of view for countries such as the U.S., which has seen a recent wave of immigration from countries with higher levels of inequality in the last decades (mainly Mexico and Central American countries). According to these models, the immigrants arriving to the U.S. from these developing countries would be drawn from the bottom of the educational distribution of their home population. Moreover, the immigrants that decide to return to their home countries would be drawn from the top of the skills distribution of the immigrant population. This would leave in the U.S. a group of permanent migrants even more negatively selected in terms of skills (see Borjas and Bratsberg (1996), pp. 167, Figure 2, for a clear illustration of this idea).

Some later work contested the previous results. Chiquiar and Hanson (2005) developed an extension of the Borjas (1991) model. They showed that if the costs of migration declines with education, then the patterns of selection might be affected. More recently, Dustmann et al. (2011) developed a model that distinguishes between two types of skills. These skills have a different price in the source and host countries and can be developed differently through experience in either country. The idea is to capture that some countries are learning centers, and that the experience gained in those countries is valuable in the host country. They concluded that immigration and return migration patterns need not to be either positively or negatively selected.

Regarding the empirical literature, there is work both consistent and inconsistent with the Borjas (1987, 1991) selection models. Recently, Fernández-Huertas Moraga (2011) and Ambrosini and Peri (2012) found evidence of Mexican immigrants' negative selection in support of this result. Chiquiar and Hanson (2005) argued that Mexican migrants are selected from the middle and up-

[1] The higher (lower) level of inequality is used as an indicator of higher (lower) returns to skills.

per section of Mexicans' wage densities. Other papers that tested the selection models include: Akee (2007), Borjas and Friedberg (2009), Feliciano (2005), Hanson (2007), Ibarraran and Lubotsky (2007), Kaestner and Malamud (2010), and Orrenius and Zavodny (2005).

Nevertheless, little empirical work has been done concerning returning migration. Borjas (1989) infers that return migration could be estimated by sample attrition using a longitudinal data set. Employing a sample of foreign-born scientists and engineers he finds that there is evidence that supports positive selection of returning migrants (i.e. the least successful leave the country). Jasso and Rosenzweig (1988) assume that migrants who do not naturalize are more likely to return to their countries, and show that the more skilled do not naturalize. Coulon and Piracha (2005) find that returning migrants to Albania are negatively selected from the country's earnings distribution. More recently, Ambrosini and Peri (2012) find positive selection of Mexican returning migrants, both in terms of observable and unobservable characteristics using a longitudinal Mexican dataset.

The data used in this paper comes from the 1970-2010 Integrated Public Use Microdata Samples (IPUMS) of the 1970 to 2000 U.S. Census, as well as the 2010 American Community Survey (ACS) (Ruggles et al., 2010). The ten source countries considered include: Canada, Central America,[2] China, Dominican Republic, Germany, India, Italy, Mexico, Philippines, and the United Kingdom. These countries were selected for their historical and contemporaneous importance as migrant populations in the U.S. Also, it was essential to include countries that had both higher and lower levels of inequality (and returns to education) to contrast the results with the predictions of selection models in the literature.

The methodology used to identify the type of selection is very simple. To assess immigrant selection, the source country's educational distribution[3] is compared to that of immigrants recently arrived to the U.S. of same-aged groups. Immigrants just-arrived are identified at each U.S. census

[2] Includes Belize, Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and Panama.

[3] The source country education distribution data is obtained from the Barro and Lee (2010) longitudinal dataset of educational attainment by age groups.

as those that report "first entering to stay in the U.S." within the last 5 years. Then, to estimate the return migration selection, synthetic cohorts are formed using the country of birth, age and "year of first entry to stay in the U.S." questions. It is assumed that changes in the education distribution of a given cohort through time are mostly explained by return migration. Given that the Census and ACS are cross-section datasets, the key assumption is that each given cohort is comparable through time. Finally, recent return migration trends are analyzed in terms of gender and age of migrants.

The analysis provides evidence of positive selection of immigrants. Interestingly, the positive selectivity of migrants has increased through time. This result is partly due to source countries' schooling improvements, but in some cases the increase in positive selectivity exceeds the source countries' progress. China, India, and Philippines are the most prominent examples of positive immigrant selection with respect to the non-migrant population's schooling distribution. Contrastingly, Mexico and Central America's positive selection from 1970 and 1980 has declined and in the case of Mexico, there is no evidence of positive selection in recent migration cohorts. No evidence of negative selection of immigrants was found for any country analyzed.

With respect to return migration, the historical analysis shows that most of the countries' early migration cohorts (i.e. those arrived 1965-1970 and 1975-1980) exhibited positive selection of immigrants staying in the United States. However, this trend has declined for later migration cohorts and, in some cases, the positive selectivity has even disappeared. Still, almost no evidence of negative selectivity of immigrants staying in the United States was found. Only a few specific subsamples of older immigrants showed some evidence of negative selectivity in terms of schooling.

The remainder of the paper is organized as follows: *Section 3.2* revises the theoretical framework that will guide the discussion; *Section 3.3* gives a brief background on U.S. migration and explains how the countries for the present analysis were chosen; *Section 3.4* describes the data used; *Section 3.5* presents the immigrants and returning migration empirical selection results; finally, *Section 3.6* concludes.

3.2 Theoretical Framework

The following model is based on the Borjas and Bratsberg (1996) theoretical framework and draws some of the components used by Chiquiar and Hanson (2005) in their extension of the Borjas (1991) model.

The main equations of the model indicate: (3.1) the income level that a person would receive in the source country, (3.2) the income level that he would receive in the host country if all the individuals from the source country were to migrate to the host country, and (3.3) the income level that he would receive as a temporary migrant if all the individuals were to temporary migrate (i.e. migrating and then returning to the source country).

$$\log w_{0i} = \mu_0 + \theta_0 s_i \tag{3.1}$$

$$\log w_{1i} = \mu_1 + \theta_1 s_i \tag{3.2}$$

$$\log w_{2i} = \lambda_i(\mu_1 + \theta_1 s_i) + (1 - \lambda_i)(\mu_0 + \theta_0 s_i + \kappa(s_i)) \tag{3.3}$$

where, the sub-indexes refer to the country of reference, being "0" the sub-index for the country of origin, "1" the sub-index for the host country, and "2" the sub-index that indicates temporary migration; w_{ji} refers to wages in country j of individual i; μ_j is the base wage; θ_j represents the returns to schooling; and s_i denotes the level of schooling of individual i. Finally, the function $\kappa(s_i)$ represents the gains that an individual with schooling s_i has on its income once he returns to his home country after migrating for λ_i proportion of time.[4]

Borjas and Bratsberg (1996) assume that the gains from migration for returning migrants on their home-country wages, $\kappa(s_i)$, are constant. There is evidence from the literature that sustains that experience gained during a migration spell might have superior returns to those gained in the host country.[5] For example, Reinhold and Thom (2009) find that Mexicans who gained experience

[4] For the time being, the form of the $\kappa(\cdot)$ function is not restricted. A more general version of the model would have the time spent abroad (λ_i) as an input of the gain function: $\kappa(s_i, \lambda_i)$.

[5] Some recent papers that provide evidence of this include: Barrett and Goggin (2010), Barrett and O'Connell (2001),

in the U.S. increased earnings more than twice compared to experience gained in Mexico. Similarly, for Irish migrants, Barrett and O'Connell (2001) find a wage premium for migration upon returning that is higher for people with post-graduate degrees.

Finally, the model includes two types of cost of migration: (i) the cost of immigration to the U.S., $\psi_M(s_i)$; and (ii) the cost of returning migration, $\psi_R(s_i)$.

In this model, an individual will choose his residence status by choosing the maximum level among three possible choices: never migrate, migrate permanently, and migrate temporally. For this model, the alternative of several temporary migrations is left out. The optimization decision can be represented by the following maximization problem:

$$Income_i = \max(\ln w_0, \ln w_1 - \psi_M(s_i), \ln w_2 - \psi_M(s_i) - \psi_R(s_i)) \tag{3.4}$$

So far, an important assumption in the model is the linearity of the returns to schooling in the log income equations for non-migrants, permanent, and temporary migrants (equations 3.1 to 3.3). Therefore, what determines the type of selection with respect to schooling is the functional form for gains from migration, $\kappa(s_i)$, and costs of migration $\psi_M(s_i)$ and $\psi_R(s_i)$, the schooling returns' parameters (θ_j) and the base wages (μ_j).

To illustrate the use of the model assume, as in Borjas and Bratsberg (1996), that the gains and costs of migration components are fixed ($\psi_M(s_i) = \psi_1$, $\psi_R(s_i) = \psi_2$, $\kappa(s_i) = \bar{\kappa}$). This implies that all the alternatives in the maximization problem represented in equation 3.4 are linear with respect to schooling. Also, assume that immigrants arrive from a country with lower returns to schooling than those of the host country, then $\theta_0 > \theta_1$. For temporary migration to be an optimal decision, it has to be the case that the gains from migration dominate the costs of migrating and re-migrating.[6] *Figure 3.1* illustrates the maximization decision under the assumption that temporary migration is an optimal enterprise for some individuals. It shows how negative selection of immigrants results

Co et al. (2000), and Iara (2006).

[6] This condition is formalized in Borjas and Bratsberg (1996), pp. 167, equation (6)

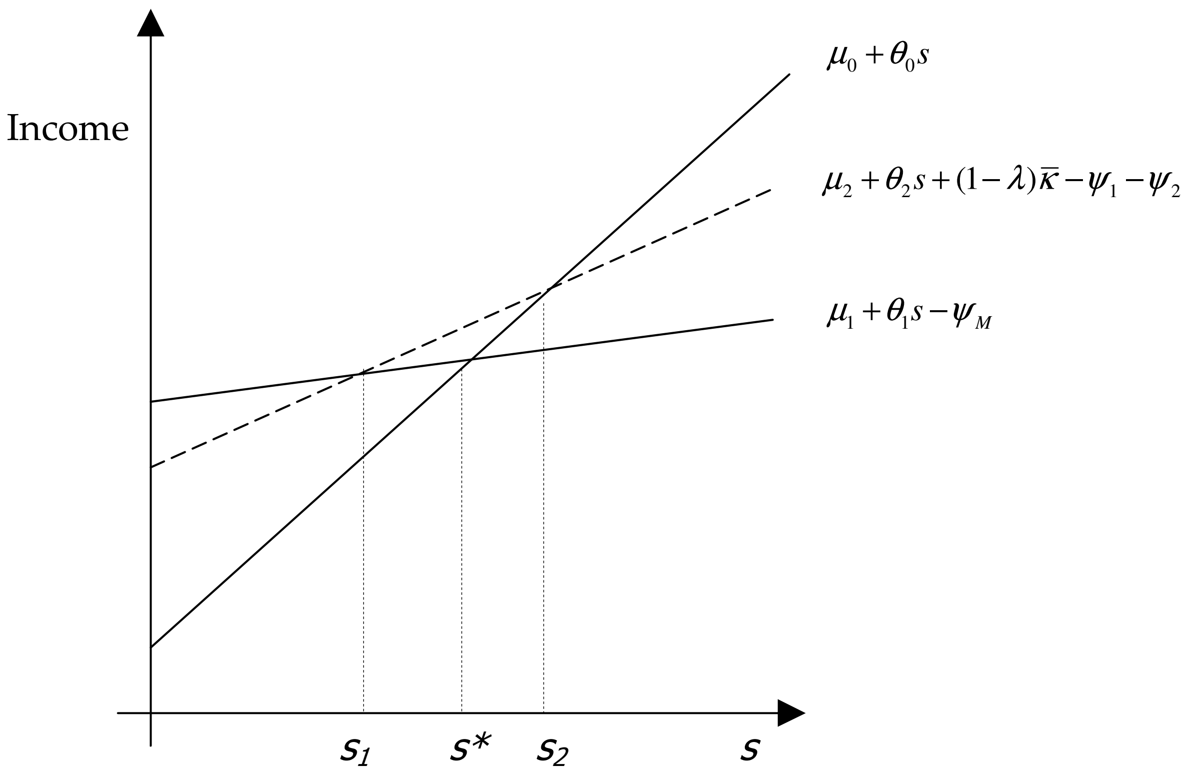


Figure 3.1: Negative selection with temporary migration

This figure assumes that $\theta_0 > \theta_1$, $\mu_1 > \mu_0$, $\psi_M(s_i) = \psi_1$, $\psi_R(s_i) = \psi_2$, $\kappa(s_i) = \bar{\kappa}$.
In the temporary migration option (dotted line), $\mu_2 = \lambda\mu_1 + (1-\lambda)\mu_0$, and $\theta_2 = \lambda\theta_1 + (1-\lambda)\theta_0$.
The optimal choice for individuals with $s_i < s_1$ is to migrate permanently, for individuals with $s_1 < s_i < s_2$ to migrate temporally, and for individuals with $s_i > s_2$ is to remain in the source country.

and how the alternative of returning migration accentuates the self-selection outcome. If returning migration was not considered, individuals below $s*$ would migrate and those above $s*$ would stay in their home country. After adding the return migration option, the individuals below s_1 decide to permanently migrate, those between s_1 and s_2 migrate temporally, and those above s_2 stay in their home country. As a result, permanent immigrants are an even more negatively selected group than without return migration (i.e. it can be easily shown formally that $s_1 < s*$).

Using this general model, it is possible to show that a different structure of the gains from migration and costs functions could yield different patterns of self-selection.[7] For instance, let

[7] For instance, Chiquiar and Hanson (2005) show that if $\psi_M(\cdot)$ is a positive but decreasing function of s, under certain

$\kappa' \leq 0$ and $\kappa'' \geq 0$, that is, let the individuals with less schooling benefit more from their experience gained during their migration spell. Also, let $\psi'_M < 0$, $\psi'_R < 0$, $\psi''_M > 0$, and $\psi''_R > 0$, that is, let the costs be a decreasing and convex function of schooling. In this case, the negative selection outcome could be overturned. To illustrate this argument in a simplified way, let the migrations cost functions be:

$$\psi_M(s_i) = \exp(\alpha_M - \phi_M s_i) \tag{3.5}$$

$$\psi_R(s_i) = \exp(\alpha_R - \phi_R s_i) \tag{3.6}$$

Furthermore, let the gains from migration be positive and constant for those individuals with schooling below $\hat{s}$, and zero for those above. Then, the gains function would be:

$$\kappa(s_i) = \bar{\kappa} \cdot 1\{s_i < \hat{s}\}, \bar{\kappa} > 0 \tag{3.7}$$

Therefore, if the gains from migration on the home income are high enough to compensate the costs of returning migration, it might be the case that for a group of individuals with low schooling and for whom permanent migration was originally their dominating option, now would be inclined toward temporary migration. This scenario is illustrated in *Figure 3.2*. All individuals below s_1 would not migrate either permanently or temporally because they face very high costs of migrating. Those between s_1 and $\hat{s}$ would find it optimal to migrate temporally. This group benefits from the gains that migration yields on their home wages. Those with schooling between $\hat{s}$ and s_2 would have a tendency towards permanent migration. This group no longer receives high enough gains from migration (i.e. none in this simplified case) to overcome the costs they have to pay for returning home. Finally, the group above s_2 remains at home. In this example, the observed self-selection pattern would depend on the support of the schooling distribution for the population in the source country.

By no means is this a general result. As described above, it is relevant to note that the self-

conditions, the negative selection of migrants might not result as Borjas (1987, 1991) predicts.

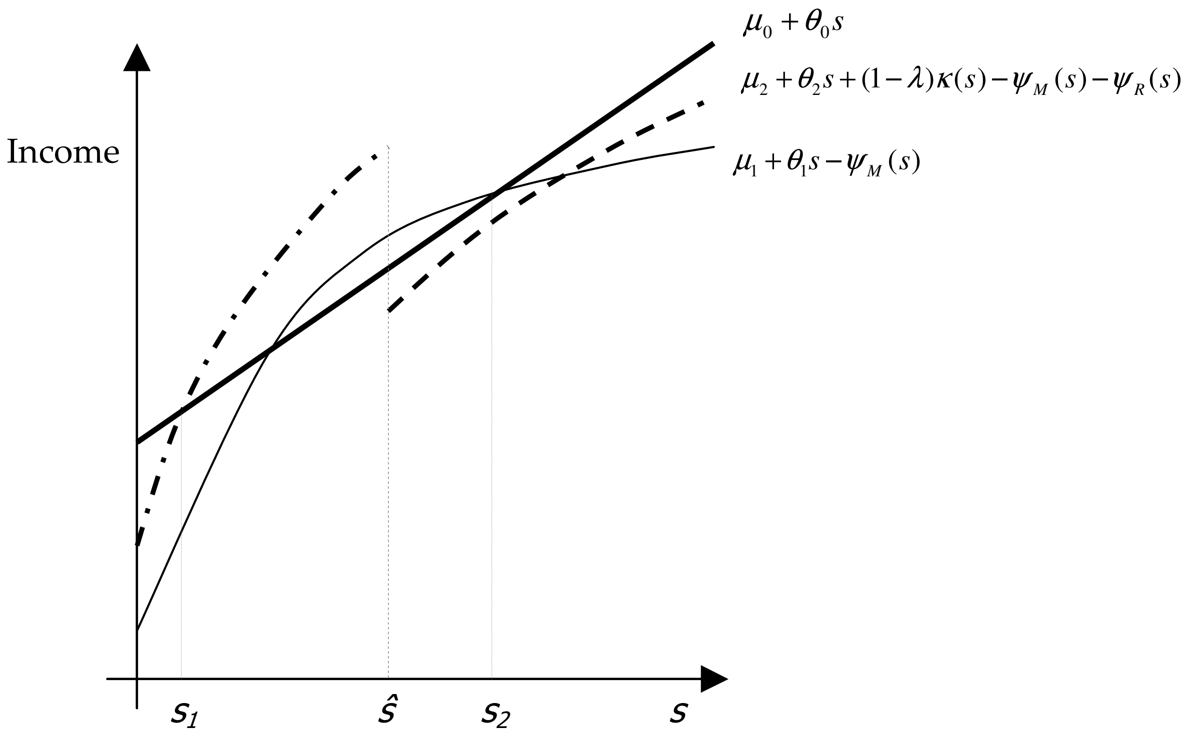


Figure 3.2: Mixed migration selection with temporary migration

This figure assumes that $\theta_0 > \theta_1$ and $\mu_0 > \mu_1$.

In the temporary migration option (dotted line), $\mu_2 = \lambda\mu_1 + (1-\lambda)\mu_0$, and $\theta_2 = \lambda\theta_1 + (1-\lambda)\theta_0$

The optimal choice for individuals with $s_i < s_1$ is not to migrate, for individuals with $s_1 < s_i < \hat{s}$ to migrate temporally, for individuals with $\hat{s} < s_i < s_2$ to migrate permanently, and for individuals with $s_i > s_2$ is to remain in the source country.

selection pattern depends entirely on the relation that costs and gains of migration have with the skills of the individuals. This relation might even be distinct between different kinds of source and host countries. For instance, the immigrants in the U.S. might get different gains from temporary migration if they arrived from Canada or Central America.

It is also important to mention that the model leaves out features that have been mentioned in the literature as having direct influence in the costs and gains from migration. For example, migration networks might have a direct implication in the costs of migration, and uncertainty over the outcome of migration might affect gains.

3.3 Selected Countries for the Analysis

The United States is, by far, the country that hosts most migrants in the world. The latest figures indicate that more than 40 million immigrants inhabit in the U.S. (Koser and Laczko, 2010). Since the end of World War II, the trend of legal immigrants admitted in the U.S. has been increasing. Recently, in 2010, 1.04 million legal immigrants were admitted (Department of Homeland Security, 2012). In addition to this, illegal immigration contributes to this numbers in a significant way as well. The latest numbers estimate the illegal population around 11.2 million migrants (Passel and Cohn, 2010).

In this paper, ten different countries were chosen to analyze the selection patterns in terms of schooling that their immigrant and returning migrant populations exhibit: Canada, Central America (Belize, Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and Panama), China, Dominican Republic, Germany, India, Italy, Mexico, Philippines, and the United Kingdom (see *Figure 3.3*).

Figure 3.3: Selected countries for the analysis
Central America includes: Belize, Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and Panama.

The following conditions were used to guide the selection of the countries for the study:

a) **Countries with important immigrant presence in the U.S.** From a practical perspective, it is relevant to know the characteristics of the population that contribute the most in absolute numbers to migration. From a methodological perspective, getting sufficient observations in the samples favors statistical validity. *Table 3.1* shows the top ten ranked countries in terms of number of migrants in the U.S. from 1970 to 2010. The countries that were top ranked in that list at some point were selected.

Table 3.1: Top source countries of immigration to the United States

Rank	1970	Pop (,000)	%	1980	Pop (,000)	%
1	**Italy**	1,009	10.5%	**Mexico**	2,199	15.6%
2	**Germany**	833	8.7%	**Germany**	849	6.0%
3	**Canada**	812	8.4%	**Canada**	843	6.0%
4	**Mexico**	760	7.9%	**Italy**	832	5.9%
5	**U.K.**	686	7.1%	**U.K.**	669	4.8%
6	Poland	548	5.7%	Cuba	608	4.3%
7	U.S.S.R.	463	4.8%	**Philippines**	501	3.6%
8	Cuba	439	4.6%	Poland	418	3.0%
9	Ireland	251	2.6%	U.S.S.R.	406	2.9%
10	Austria	214	2.2%	Korea	290	2.1%
Rank	1990	Pop (,000)	%	2000	Pop (,000)	%
1	**Mexico**	4,298	21.7%	**Mexico**	9,177	29.5%
2	**Philippines**	913	4.6%	**Philippines**	1,369	4.4%
3	**Canada**	745	3.8%	**India**	1,023	3.3%
4	Cuba	737	3.7%	**China**	989	3.2%
5	**Germany**	712	3.6%	Vietnam	988	3.2%
6	**U.K.**	640	3.2%	Cuba	873	2.8%
7	**Italy**	581	2.9%	Korea	864	2.8%
8	Korea	568	2.9%	**Canada**	821	2.6%
9	Vietnam	543	2.7%	**El Salvador**	817	2.6%
10	**China**	530	2.7%	**Germany**	707	2.3%
Rank	2010	Pop (,000)	%			
1	**Mexico**	11,711	29.3%			
2	**India**	1,780	4.5%			
3	**Philippines**	1,778	4.4%			
4	**China**	1,608	4.0%			
5	Vietnam	1,241	3.1%			
6	**El Salvador**	1,214	3.0%			
7	Cuba	1,105	2.8%			
8	Korea	1,100	2.8%			
9	**Dom. Rep.**	879	2.2%			
10	**Guatemala**	831	2.1%			

Source: U.S. 1970, 1980, 1990, and 2000 Census and 2010 American Community Survey.

b) **Countries with both higher and lower levels of inequality (or human capital returns) than the U.S.** Given the theoretical framework specified in *Section 3.2*, this information would predict if $\theta_0 \gtrless \theta_1$. If the costs and gains functions are assumed to be fixed, then this information should be sufficient to predict the patterns of self-selection in terms of schooling. *Figure 3.4* shows the difference of the GINI coefficient between the U.S. and the selected countries whenever possible (United Nations, 2008).[8] Additionally, *Figure 3.5* shows the difference in the returns to schooling between the U.S. and the selected group of countries, whenever the data was available.[9]

c) Geography. A natural choice was to select the two U.S. bordering countries: Canada and Mexico. In addition to being both bordering countries, they have the opposite relation with respect to the U.S. in terms of inequality. Hence, it would be interesting to compare their immigrants patterns of selection in terms of schooling. Central American countries and the Dominican Republic are the next group of countries in terms of proximity. Additionally, their levels of inequality might make a comparable case to Mexican migration. The Central American countries share borders, similar levels of inequality and schooling distribution within each other.

3.4 Data and Empirical Strategy

3.4.1 Data

The data used in this paper comes from two main sources:

Barro and Lee (2010). The educational attainment from the source countries comes from the Barro and Lee (2010) panel dataset (B&L hereafter). This is a longitudinal dataset on educational attainment that covers 146 countries from 1950 to 2010. The information is provided every 5 years and each country's population is disaggregated by gender and in 5-year age intervals. All the countries chosen for the analysis have available information. Educational attainment is classified in seven categories: (i) no schooling; (ii) primary incomplete; (iii) primary complete; (iv) secondary

[8] The dataset used for this comparison is the WIID2b from the United Nations. This dataset collects information from national surveys. In particular, the income definitions used to construct the GINI coefficients are usually different among countries. The WIID2b database pays special attention to this problem to favor comparability among countries. For more information see: `http://www.wider.unu.edu/research/Database/en_GB/database`

[9] The data for returns to schooling comes from Psacharopoulos and Patrinos (2002)

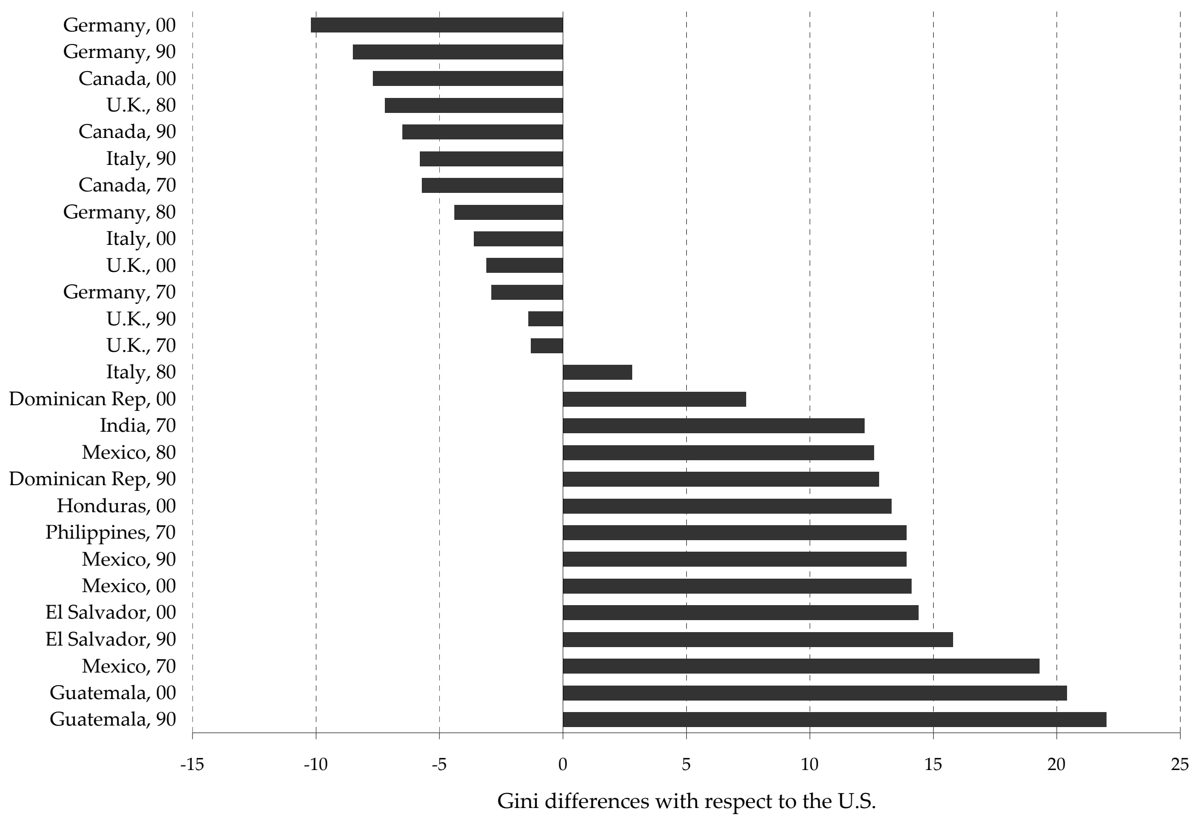


Figure 3.4: GINI index differences of source countries with respect to the United States

GINI index is measured between 0 and 100.
A positive (negative) value indicates that the source country is more (less) unequal than the United States. According to the theoretical framework described in *Section 3.2*, this should help predict the type of selectivity of immigrants and returning migrants.
Source of GINI indexes: UNU-WIDER dataset (United Nations, 2008).

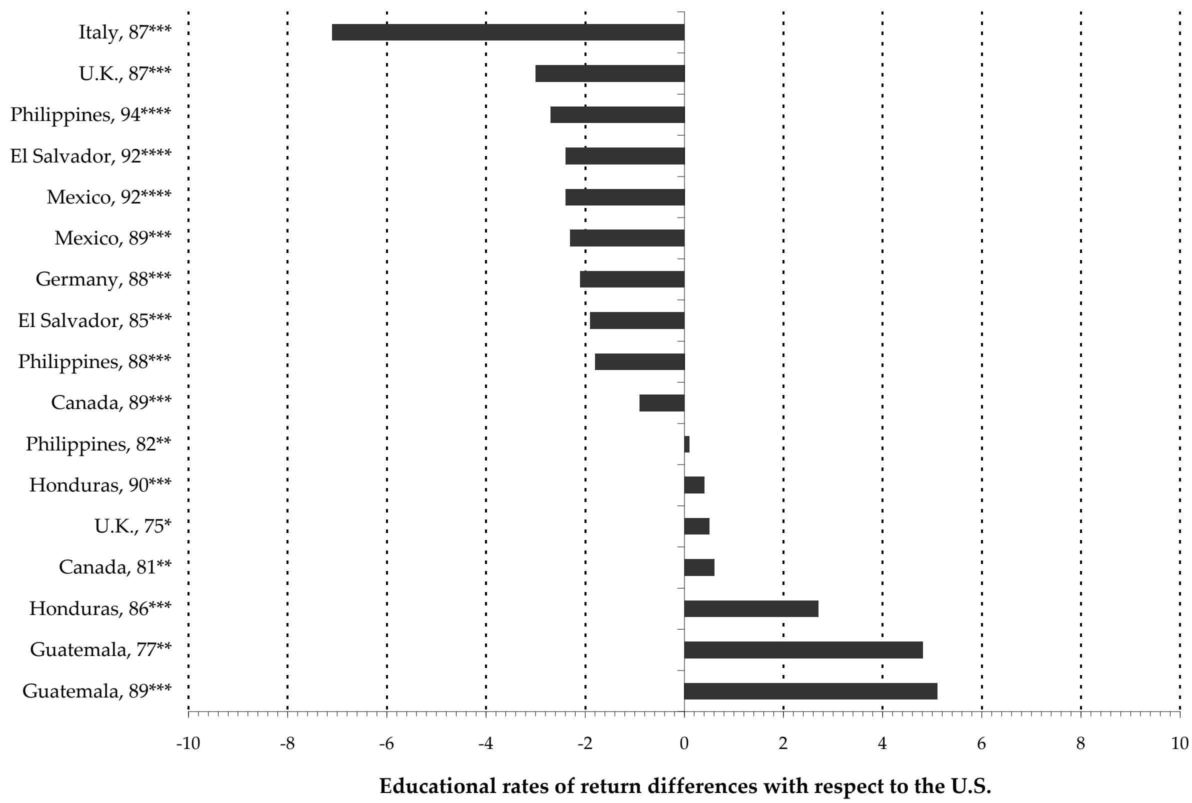


Figure 3.5: Educational rates of return differences of source countries with respect to the United States

Differences are in percentage points. A positive (negative) value indicates that the source country has higher (lower) rates of return to years of schooling than the United States.
Source of rates of return to schooling: Psacharopoulos and Patrinos (2002).

incomplete; (v) secondary incomplete; (vi) tertiary incomplete; and (vii) tertiary complete. The B&L dataset was constructed with the specific purpose of cross-country comparisons and has been constantly been updated and improved. See Barro and Lee (2010) for further details.

Integrated Public Use Microdata Samples (IPUMS). The U.S. 1970-2000 U.S. Decennial census 5% samples (except for 1970, where the 1% Form 1 State sample was used), and the 2010 American Community Survey (ACS) are used in the analysis. The analysis presented here will use individual information about school attainment, immigrants' country of birth, first year of entry to stay in the U.S., citizenship status, and labor market indicators. The analysis is restricted to individuals not living in group quarters, that report being in the labor force, with positive individual income during the previous year, inhabiting in the continental U.S. territory.[10]

3.4.2 Empirical strategy

Identification of immigrant selection. At each census, a group of recently arrived immigrants are identified as those individuals born in a foreign country that report first entering to stay in the U.S. in any of the five years previous to that census (e.g. in the case of the 1970 census, a recent arrived immigrant would have entered the U.S. between 1965 and 1970 for the first time).[11] It is assumed that individuals report their first entrance to the United States with the purpose of inhabiting there and not for temporary stays (like vacation or family visits).

Immigrant selection is analyzed in terms of schooling. Recently arrived immigrants are compared to the same-aged population from their source country at each census: 1970, 1980, 1990, and 2000 on a country-by-country basis. The source country schooling distribution comes from the B&L dataset. To match the schooling attainment categories available form the B&L dataset, a standardized variable of school attainment is generated using the categories available at the census datasets. *Table B.2* in the *Appendix B* details how the standardized educational variable was formed using the schooling categories available at the different census years.

[10] Those individuals with total personal income above the 99th percentile are excluded to avoid outliers and those living in Hawaii or Alaska are also excluded since migration trends might be different to those locations.

[11] The 1970, 1980, and 1990 censuses asked when the person first came to stay in the U.S.; the 2000 census and the ACS asked when the person first came to live in the United States.

Identification of return migrant selection. Recently arrived migrants are followed though the different census years by using synthetic cohorts. The cohorts are defined based on the country of birth, age, and "first year of entry to stay in the U.S." Each cohort is followed through three census years. Ages are restricted to recently arrived migrants over 30 to avoid individuals that migrate to the U.S. and acquire additional education while in the U.S. Also, ages are restricted below 65 at the last cohort follow-up. The following table gives a clear illustration of how the cohorts are formed:[12]

Cohort definitions

Cohort	Year of Entry to the U.S.	Ages				
		1970	1980	1990	2000	2010
1	1965-1970	30-45	40-55	50-65		
2	1975-1980		30-45	40-55	50-65	
3	1985-1990			30-45	40-55	50-65
4	1995-2000				30-45	40-55

To identify the selection in terms of schooling of return migrants, the distribution of the educational attainment is compared for a given cohort through the different census years. The data is not longitudinal so the results should be interpreted as how does the schooling distribution changes for groups of people with similar baseline characteristics observed at different points in time. Differences in the distribution are assumed to be mainly the result of migrants returning to their country of origin. Therefore, if a given cohort's schooling distribution reflects a more (less) educated group 20 or 10 years after the initial migration, it is assumed that the migrants from that group that left were less (more) educated.

One concern from the analysis is that migrants might poorly report the "first year of entry to the U.S." question. More educated individuals might be more likely to have previously visited the U.S. if they are from a foreign country. As a result, they might be undercounted as recently arrived immigrants (1970, 1980 and 1990 census), but not in a follow-up where the text of the "first year of entry" question changed to explicitly include "first entry to live in the U.S." (after the 2000 census and the ACS). This might bias the analysis towards positive selection. Only the analysis of cohort 4 would not be affected by this potential bias.

[12] *Table B.1* in the *Appendix B* indicates the number of observations for each cohort by census (or ACS) year and country of origin.

Recent returning migration trends. A more detailed analysis of recent return migration trends is done with post-2000 data. *Table B.2* in the *Appendix B* indicates how a more detailed schooling variable is formed using the 2000 census and 2010 ACS schooling categories. Given that the cohort follow-up only considers a 10-year window, the age restriction for the recently arrived migrants is modified to individuals aged 25-54 in 2000. This analysis will also consider differences in returning migration trends for males and for younger (individuals aged 25-39 in 2000) versus older immigrants (individuals aged 40-59 in 2000). The younger versus older cohort analysis will give some insight of to what extent the differences in schooling distributions through time might be related to deceased individuals rather than returning migrants.

Limitations. Other confounding explanations include that differences might also arise from migrants moving to other destinations (different form their country of origin). Also, it is still possible that some adults acquire some type of education after their initial migration. Finally, the age restriction leaves out individuals that might migrate to the U.S. to acquire tertiary education (undergraduate or graduate level schooling). This restriction will diminish the positive selection of immigrants to the U.S. for some countries in the analysis. The investigation provided here only reflects immigration decisions of individuals that had completed their schooling in a country excluding the U.S.

3.5 Results

The results presented in this section attempt to shed some light on three subjects: (i) the historical selection patterns observed in terms of schooling for just-arrived immigrants in the U.S. compared to their same-aged home country population; (ii) the historical selection patterns observed in terms of schooling for permanent migrants in the U.S. with respect to migrants that left; and (iii) the recent selection patterns in terms of schooling of returning migration immigrants distinguishing for gender and age. Given the information about inequality and returns to education (*Figures 3.4* and *3.5*), it is also possible to evaluate to what extent are the predictions from the theoretical literature met. In addition, the evidence provided will give detailed information to analyze if there are any patterns or trends for each of the ten countries under study.

3.5.1 Selection of immigrants

To determine the pattern of selection of immigrants with respect to their home country's population, the cumulative distribution function (hereafter CDF) of recently arrived immigrants' schooling is compared to that of their same-aged home-country population. *Figures 3.6a* to *3.6j* illustrate this comparison in a country-by-country basis for each of the cohorts previously defined. The bold line on each figure shows the schooling CDF of the source country's population with ages 30 to 44, obtained from the B&L dataset. The dashed line shows the schooling CDF of the "just arrived" immigrant population. Finally, the gray line shows the schooling CDF of the latest follow-up available for each cohort. The latter line will be used to assess the return migrants' selectivity. For example, in the first panel of *Figure 3.6a (Cohort 1)*, the bold line shows the 1970's schooling CDF of the Canadian population 30-44 years old; the dashed line shows the 1970's schooling CDF of Canadian immigrants who arrived to the U.S. between 1965 and 1970 and were between 30 to 44 years old; and the gray line shows the 1990's schooling CDF of Canadian immigrants that arrived to the U.S. between 1965 and 1970 and were between 50 and 64 years old.

Whenever the home-country CDF first-order dominates the just arrived immigrants CDF, there would be evidence of negative selection. The first-order dominance would indicate that for any category of schooling, there would be a higher proportion of home country's population than just-arrived migrants with more or equal schooling. In the opposite case, whenever the just-arrived migrants CDF first-order dominates, there would be evidence for positive selection.

Figures 3.6a to *3.6j* provide overwhelming evidence of positive selection in terms of schooling in almost every cohort-country case. China, India, and Philippines exhibit the largest differences between the source country and the recent immigrants' CDFs, being all cases of positive selection. For example, *Figure 3.6f* shows that most of India's population attain levels of school achievement below complete secondary level. Nevertheless, over 80% of the migrants that arrive to the U.S. show the highest level of schooling (tertiary complete).

The positive selectivity of just arrived immigrants is evident even for countries with higher levels of inequality and returns to schooling than the U.S. (like Central America and Dominican Republic). The only cases where there is no first order domination of the immigrants CDF are Italy (cohort 1) and Mexico (cohorts 2 to 4). However, none of these cases shows evidence of negative selectivity either. The case of Mexico is of particular relevance, given that nowadays it is the main contributor of foreign-born population in the U.S. For the most recent immigration wave analyzed, 40% of the recent arrived Mexican migrants in 2000 had levels of schooling below or equal to primary complete (*Figure 3.6h*). The only case that is close to this proportion is Central America (39%), however, the Central America's source country schooling lags significantly below the Mexican.

Finally, the positive selectivity of just arrived immigrants has increased through time, mainly because of the increase in the level of schooling at each source country. Still, in some cases the increase in positive selection of recently arrived immigrants has exceeded the increase in their countries' level of schooling. For example, Canada, China, and Italy's positive selectivity increased through time, whereas Central America, Mexico, and Philippines decreased.

This evidence of positive selection is not particular for the age range used. Very similar results are obtained if the age range is expanded or reduced. It could be argued that the home-population distribution includes soon-to-be immigrants and returned migrants. However, for the argument of soon-to-be immigrants to overturn the result, it would have to be the case that immigrants with low levels of schooling make their migration decision rather late. Even if this was the case, a large number of individuals would be needed to overturn the result for most of the countries analyzed.

3.5.2 Selection of returning migrants

To determine the selection patterns that result from returning migration, the synthetic cohorts are followed through time. As described above, the gray solid lines on *Figures 3.6a* to *3.6j* represent the CDF for the population that remains in the U.S. 20 to 25 years after their initial immigration. To examine the return migration selection patterns, this CDF will be compared within each cohort to the CDF of the immigrants when they were recently-arrived (the dashed line). Following the idea

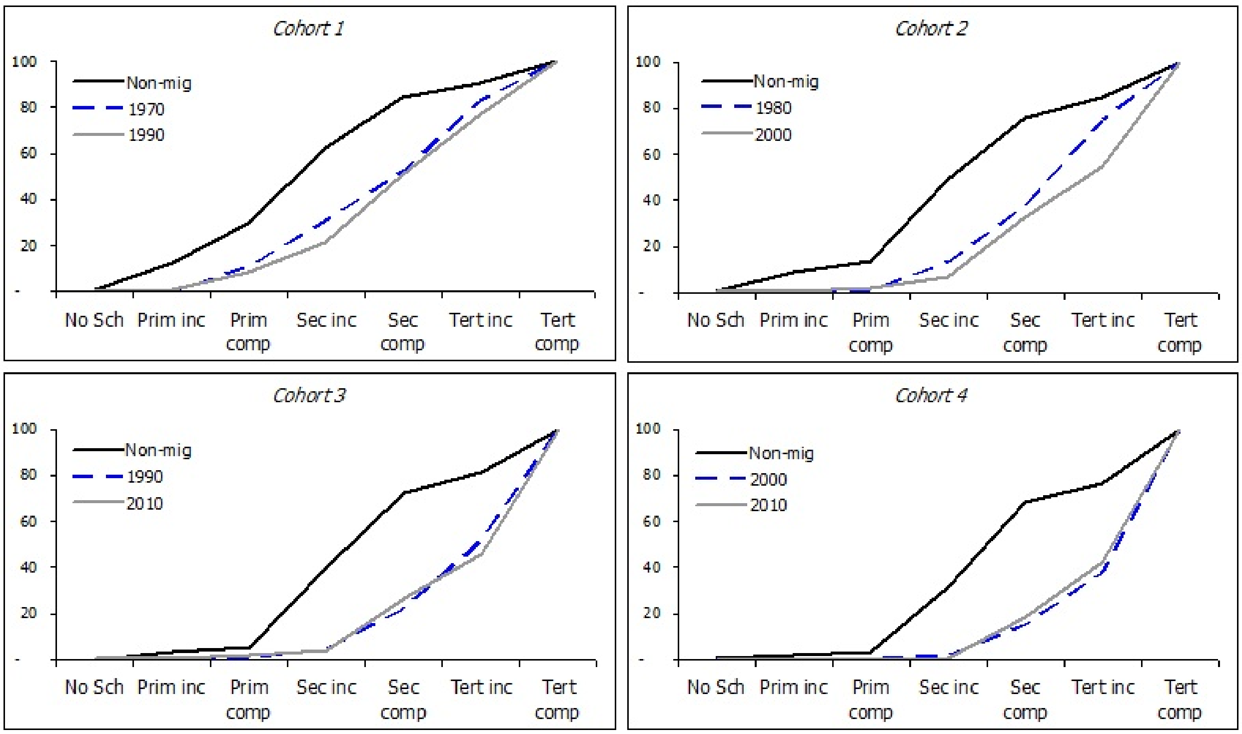


Figure 3.6a: Immigrant and return migration. CANADA

Cohort 1: Immigrants arrived 1965-1970.

Cohort 2: Immigrants arrived 1975-1980.

Cohort 3: Immigrants arrived 1985-1990.

Cohort 4: Immigrants arrived 1995-2000.

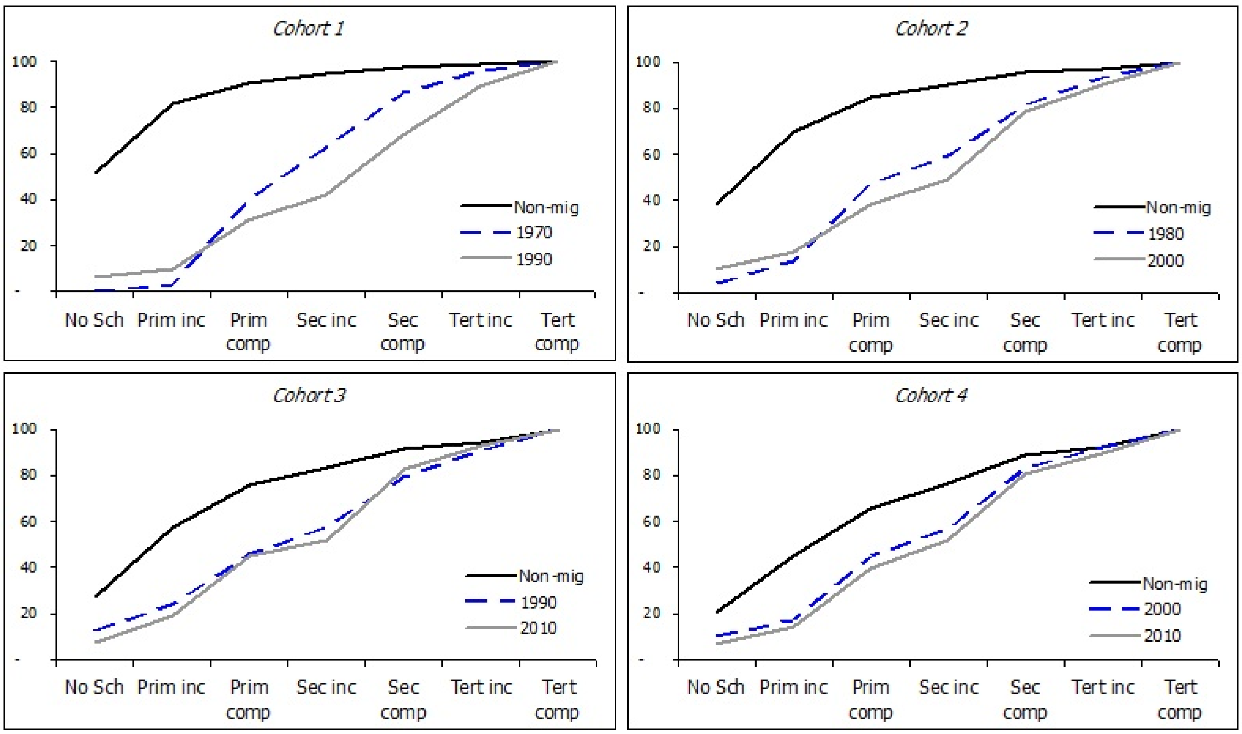


Figure 3.6b: Immigrant and return migration. CENTRAL AMERICA

Central America includes: Belize, Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and Panama.

Cohort 1: Immigrants arrived 1965-1970.

Cohort 2: Immigrants arrived 1975-1980.

Cohort 3: Immigrants arrived 1985-1990.

Cohort 4: Immigrants arrived 1995-2000.

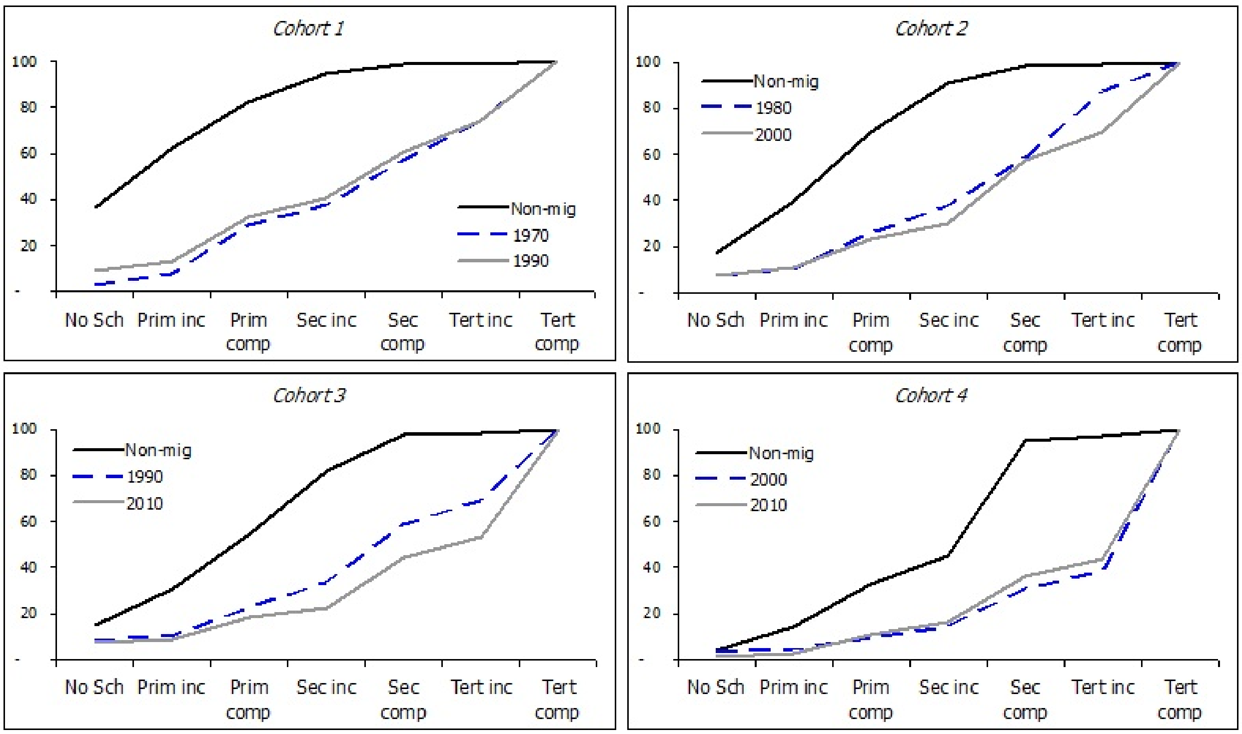


Figure 3.6c: Immigrant and return migration. CHINA

Cohort 1: Immigrants arrived 1965-1970

Cohort 2: Immigrants arrived 1975-1980

Cohort 3: Immigrants arrived 1985-1990

Cohort 4: Immigrants arrived 1995-2000

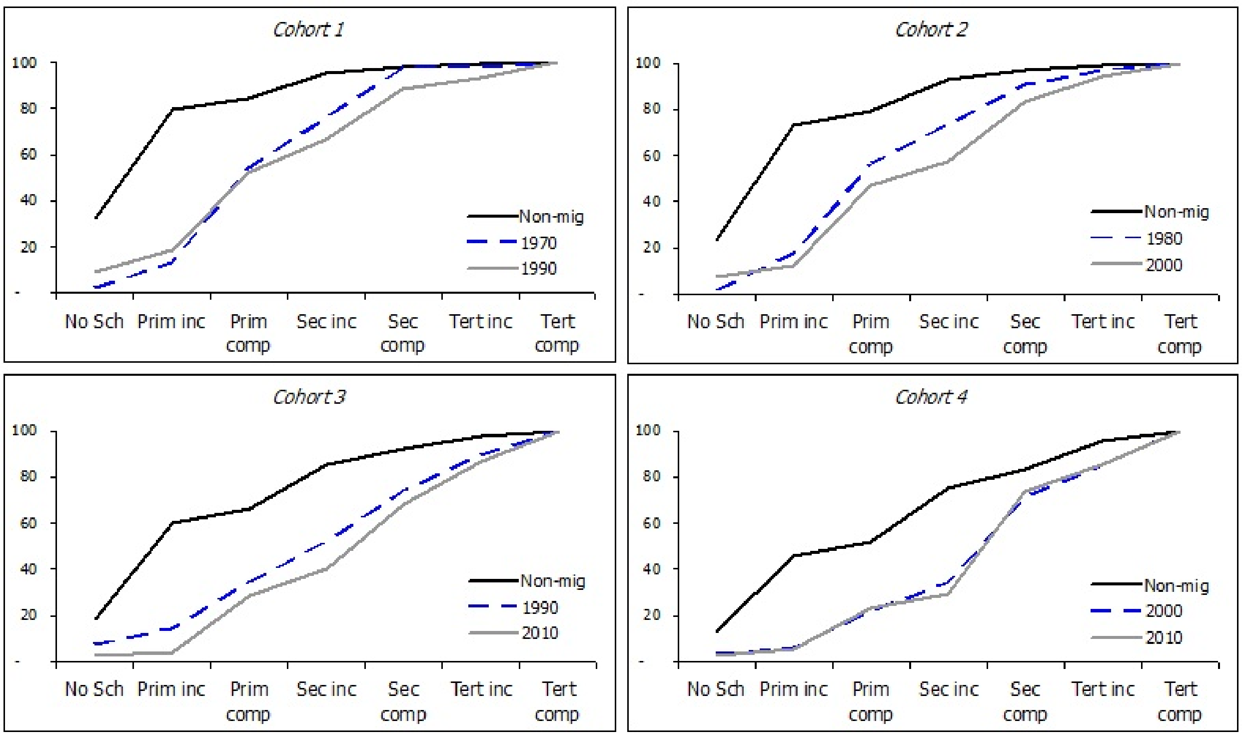


Figure 3.6d: Immigrant and return migration. DOMINICAN REPUBLIC

Cohort 1: Immigrants arrived 1965-1970

Cohort 2: Immigrants arrived 1975-1980

Cohort 3: Immigrants arrived 1985-1990

Cohort 4: Immigrants arrived 1995-2000

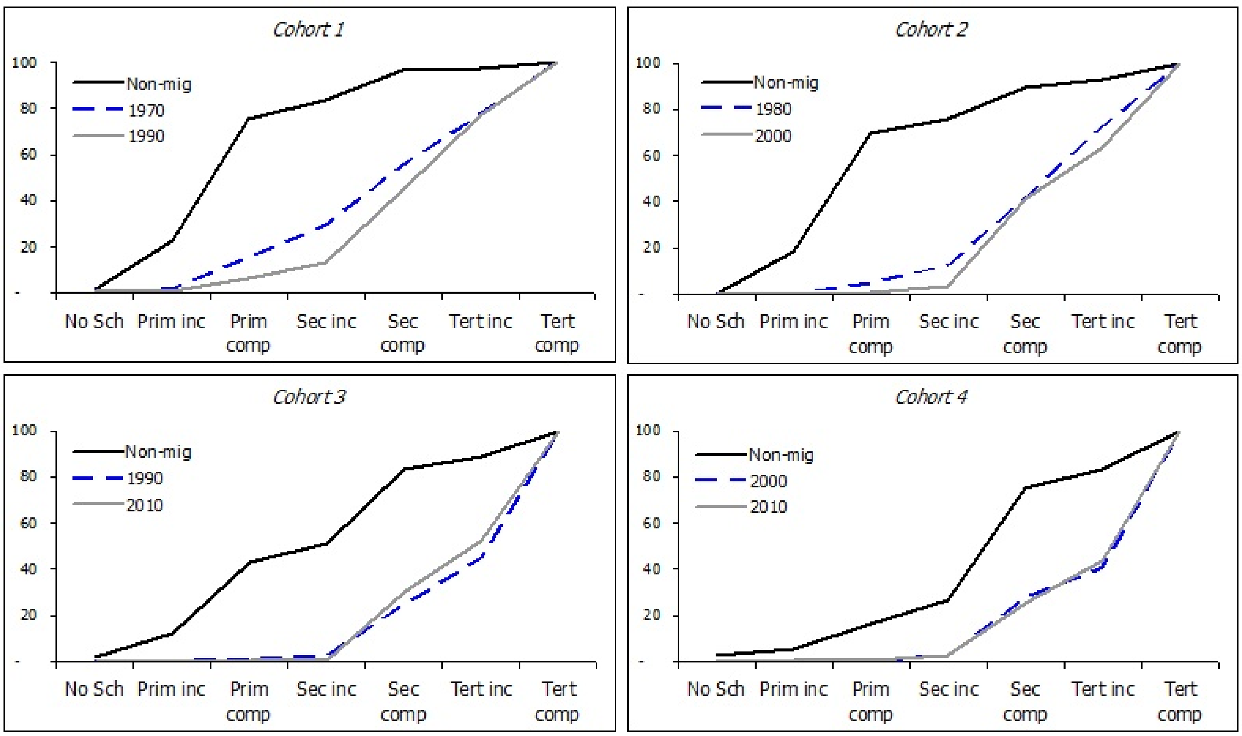


Figure 3.6e: Immigrant and return migration. GERMANY

Includes Eastern and Western Germany previous to 1990.

Cohort 1: Immigrants arrived 1965-1970

Cohort 2: Immigrants arrived 1975-1980

Cohort 3: Immigrants arrived 1985-1990

Cohort 4: Immigrants arrived 1995-2000

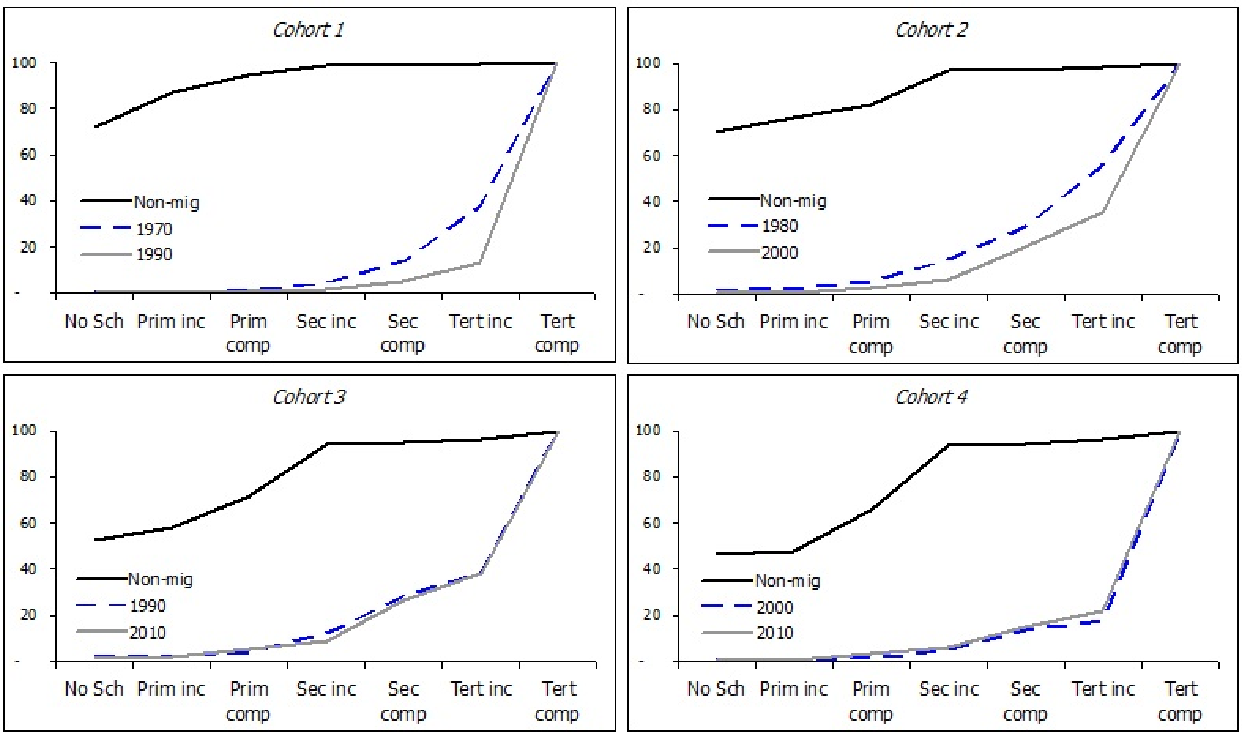


Figure 3.6f: Immigrant and return migration. INDIA

Cohort 1: Immigrants arrived 1965-1970

Cohort 2: Immigrants arrived 1975-1980

Cohort 3: Immigrants arrived 1985-1990

Cohort 4: Immigrants arrived 1995-2000

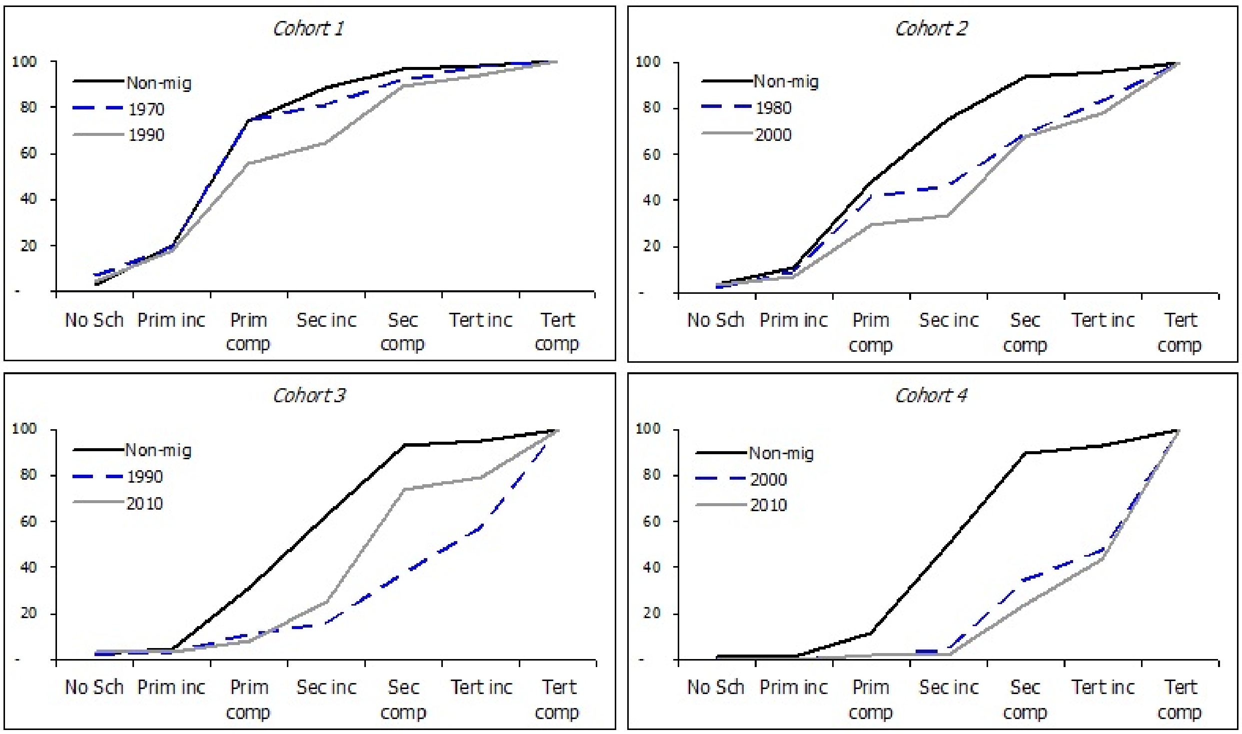


Figure 3.6g: Immigrant and return migration. ITALY

Cohort 1: Immigrants arrived 1965-1970

Cohort 2: Immigrants arrived 1975-1980

Cohort 3: Immigrants arrived 1985-1990

Cohort 4: Immigrants arrived 1995-2000

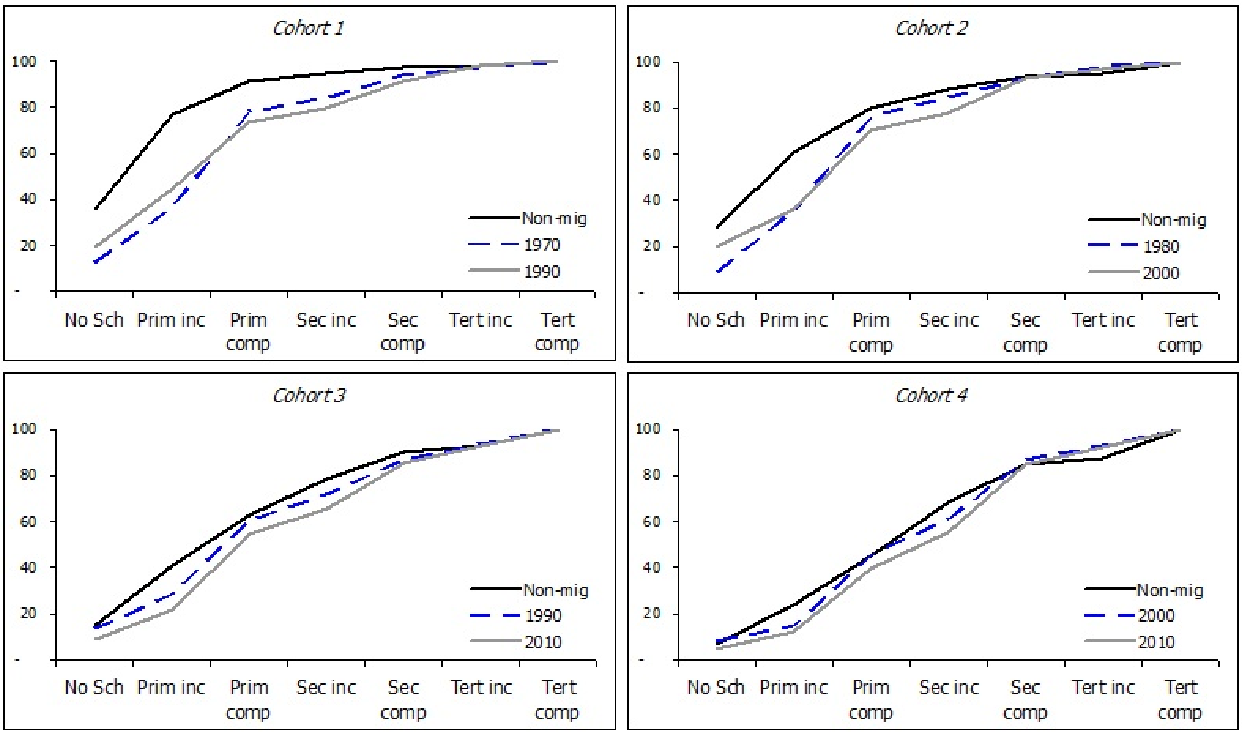


Figure 3.6h: Immigrant and return migration. MEXICO

Cohort 1: Immigrants arrived 1965-1970

Cohort 2: Immigrants arrived 1975-1980

Cohort 3: Immigrants arrived 1985-1990

Cohort 4: Immigrants arrived 1995-2000

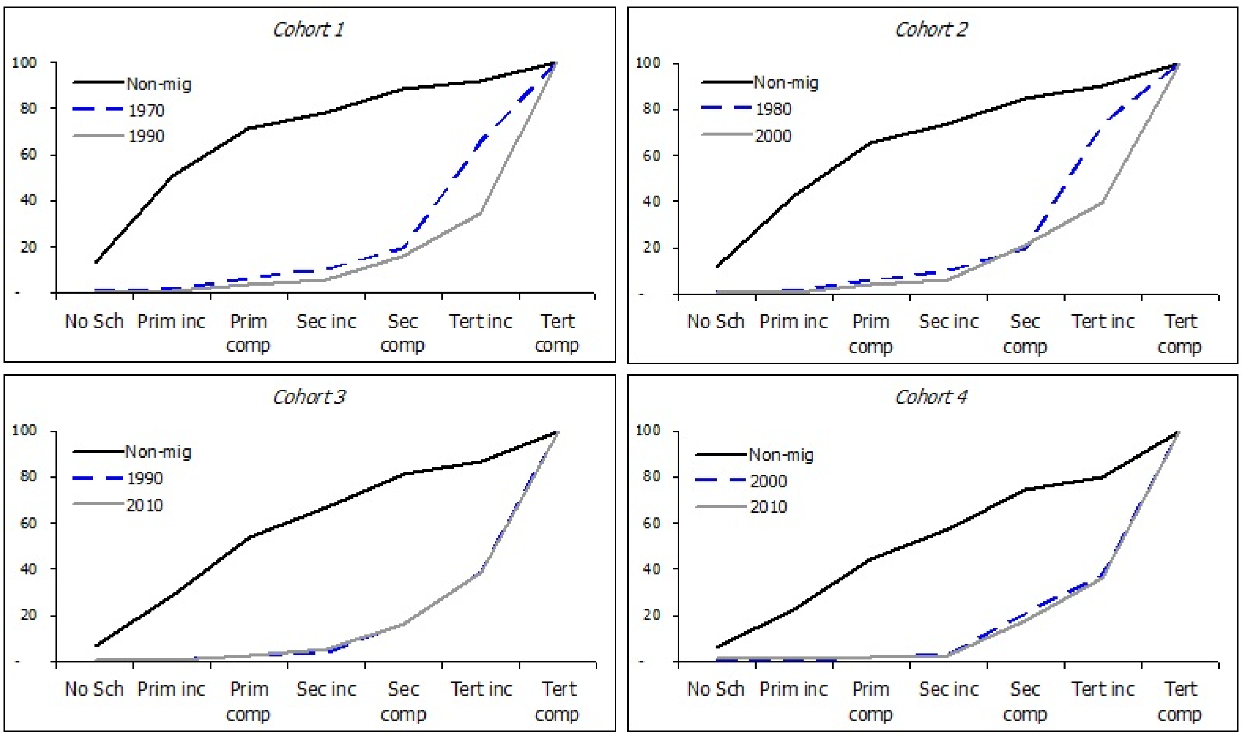


Figure 3.6i: Immigrant and return migration. PHILIPPINES

Cohort 1: Immigrants arrived 1965-1970

Cohort 2: Immigrants arrived 1975-1980

Cohort 3: Immigrants arrived 1985-1990

Cohort 4: Immigrants arrived 1995-2000

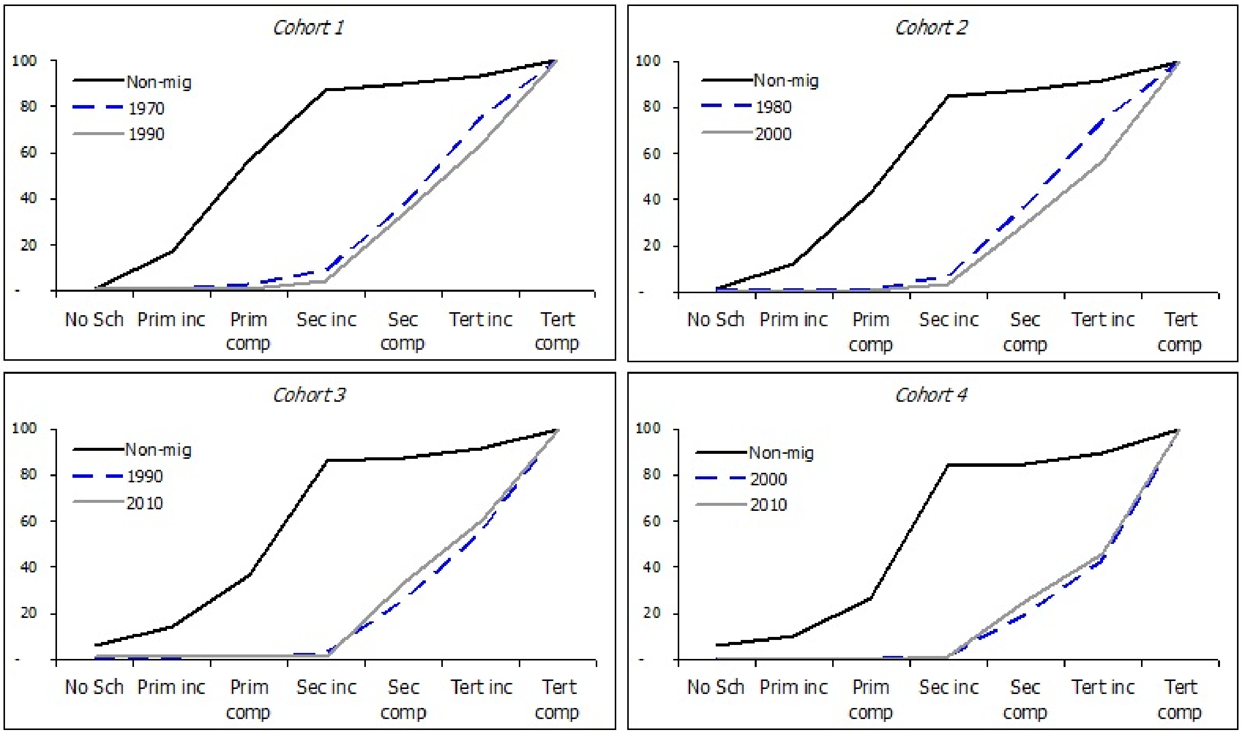


Figure 3.6j: Immigrant and return migration. UNITED KINGDOM

Cohort 1: Immigrants arrived 1965-1970

Cohort 2: Immigrants arrived 1975-1980

Cohort 3: Immigrants arrived 1985-1990

Cohort 4: Immigrants arrived 1995-2000

in Borjas (1989) that outmigration behavior can be inferred from sample attrition in a longitudinal data set of foreign-born scientists and engineers, it is assumed here that the main determinant of the difference between these CDFs is the population attrition originated by returning migrants.

If the CDF of the just-arrived immigrants (dashed line) first-order dominates the CDF of the immigrants that remain after several years (gray solid line), there would be evidence of negative selection of the immigrant population that stays in the U.S. On the contrary, if the CDF of the immigrants that remain first-order dominates, there would be evidence in favor of positive selection.

Most cases illustrated in *Figures 3.6a* to *3.6j*, suggest that positive selection in terms of schooling dominate in earlier cohorts (Cohorts 1 and 2). Still, the positive selection magnitudes are not closely as sizeable as the immigrants' arrival selection. This positive selectivity usually disappears or at least is greatly reduced in recent cohorts (particularly Cohort 4). Yet, there is very limited evidence of negative selection of staying migrants. The evidence presented, paired with the levels of inequality and returns to education, is not consistent with the theoretical framework the prediction of the model with constant costs and gains from migration.

Table 3.2 also provides interesting insight from the descriptive statistics. In particular, it is notable that through time the proportion of males tends to decrease in all the countries' cases. This suggests that female migration tends to be more permanent than males. The individuals most successful to gain naturalization ten years after their initial arrival to the U.S. are from China, India, and Philippines, which on average achieve U.S. citizenship in 58%, 56%, and 70% of the cases ten years after the initial immigration, respectively. On contrast, the geographically closest countries, Mexico, Central America, and Canada have the lowest level of naturalization ten years after the initial migration, with 20%, 25%, and 27% proportions on average, respectively. This might reflect that those are also the countries more prone to temporary or circulatory migration.

In terms of labor market outcomes, the immigrants with higher levels of unemployment are those from Dominican Republic and Central America. Unemployment levels peaked in the recent era. Finally, to give some insight of the relative position of immigrants in terms of earnings, the

Table 3.2: Descriptive statistics for each country by year and cohort

		Country									
Cohort	Year	CAM	CAN	CHI	D.R.	ENG	GER	IND	ITA	MEX	PHI
		a. Proportion of male immigrants									
1	1970	0.49	0.77	0.72	0.52	0.78	0.69	0.82	0.65	0.68	0.56
1	1980	0.38	0.58	0.59	0.52	0.61	0.43	0.79	0.66	0.63	0.47
1	1990	0.45	0.55	0.56	0.61	0.55	0.39	0.74	0.67	0.62	0.44
2	1980	0.53	0.65	0.6	0.58	0.71	0.66	0.68	0.73	0.74	0.46
2	1990	0.51	0.51	0.58	0.6	0.6	0.36	0.65	0.65	0.67	0.43
2	2000	0.47	0.5	0.56	0.6	0.61	0.39	0.62	0.59	0.63	0.42
3	1990	0.59	0.62	0.54	0.54	0.7	0.61	0.69	0.71	0.74	0.42
3	2000	0.52	0.56	0.55	0.55	0.65	0.45	0.66	0.63	0.64	0.4
3	2010	0.53	0.4	0.54	0.55	0.51	0.41	0.62	0.71	0.65	0.36
4	2000	0.65	0.63	0.57	0.5	0.73	0.62	0.74	0.66	0.71	0.43
4	2010	0.57	0.57	0.54	0.49	0.68	0.48	0.66	0.7	0.63	0.41
		b. Proportion of naturalized immigrants									
1	1970	0.15	0.06	0.15	0.28	0	0.12	0.06	0.1	0.24	0.07
1	1980	0.36	0.27	0.65	0.26	0.39	0.44	0.56	0.51	0.21	0.75
1	1990	0.52	0.34	0.83	0.47	0.47	0.54	0.73	0.62	0.29	0.89
2	1980	0.08	0.05	0.06	0.14	0.01	0.1	0.07	0.12	0.12	0.09
2	1990	0.27	0.18	0.72	0.24	0.19	0.34	0.56	0.39	0.24	0.78
2	2000	0.56	0.44	0.86	0.48	0.42	0.47	0.81	0.58	0.36	0.9
3	1990	0.06	0.09	0.06	0.12	0.06	0.12	0.05	0.13	0.11	0.11
3	2000	0.21	0.3	0.54	0.35	0.29	0.43	0.54	0.47	0.23	0.66
3	2010	0.37	0.45	0.84	0.6	0.6	0.42	0.84	0.61	0.34	0.85
4	2000	0.06	0.1	0.05	0.08	0.12	0.27	0.04	0.14	0.07	0.12
4	2010	0.18	0.33	0.42	0.4	0.35	0.29	0.57	0.35	0.14	0.64

Source: U.S. 1970, 1980, 1990, and 2000 Census and 2010 American Community Survey

Table 3.2a: Descriptive statistics for each country by year and cohort (cont.)

Cohort	Year	CAM	CAN	CHI	D.R.	ENG	GER	IND	ITA	MEX	PHI
		c. Percentage of immigrants unemployed									
1	1970	2.8	2.1	4.3	13.0	2.8	1.7	2.3	4.5	6.2	0.7
1	1980	5.1	2.8	3.4	7.5	2.1	3.4	2.6	8.4	8.4	2.4
1	1990	4.2	3.7	3.5	9.0	3.6	2.2	3.8	7.6	11.5	3.2
2	1980	5.8	2.6	2.4	7.0	2.5	5.0	4.7	7.0	8.2	2.9
2	1990	6.4	1.9	2.6	10.5	0.9	3.1	2.0	3.0	9.9	3.1
2	2000	5.7	2.5	3.5	12.5	2.7	1.0	2.5	3.6	8.2	3.5
3	1990	7.1	3.1	4.5	11.0	3.0	2.8	5.5	5.1	8.0	2.9
3	2000	6.2	1.5	3.0	10.0	2.0	1.9	2.3	1.8	7.3	2.8
3	2010	10.9	6.5	9.1	11.0	1.1	13.9	4.5	7.8	9.3	6.0
4	2000	6.0	1.6	3.3	9.7	1.5	1.3	2.4	2.7	5.9	3.1
4	2010	8.2	3.1	3.0	9.3	1.9	4.3	3.6	5.3	7.3	4.6
		d. Median percentile of the total personal income[a]									
1	1970	37.1	77.4	36.4	34.3	86.8	70.9	60.75	46.35	30.3	38.1
1	1980	39.4	71.9	45	36.1	77.55	61.6	90	55.3	38.3	66.2
1	1990	47.5	72.9	47.5	42.25	76.8	60.1	90.9	63.8	34	72.3
2	1980	28.1	75.2	34.3	26.3	73.7	68.7	55.3	54.3	31.2	45
2	1990	34	72.9	49.3	34	72.9	60.1	75.7	59.9	30.4	61.3
2	2000	32.3	73.6	40.7	29.8	76.3	59.9	75.7	55.5	28.7	59.9
3	1990	23.6	72.9	27	29.4	72.9	64	46.6	62	22.1	42.5
3	2000	29.8	70.5	52	30	79.1	59.9	62.8	66.1	28.3	55.5
3	2010	31	70.5	50.1	34.7	64.8	60	66.1	76.9	31	60.2
4	2000	24.3	77.8	45.9	25.4	80.5	68.4	73.6	61.6	22.6	41
4	2010	30.3	78.2	64.5	31	83.9	66.1	80.9	77.4	27.9	54.2

Source: U.S. 1970, 1980, 1990, and 2000 Census and 2010 American Community Survey.

[a] Median percentile income is calculated with respect to the full U.S. employed population over 15 years old.

percentile of total individual pre-tax income the previous year to the data collection is calculated with respect to the full U.S. population. The median is reported for each country-cohort-year group. Immigrants from India stand out since they begin with high levels of relative income and increase their relative position through time for all cohorts. Immigrants from Canada, U.K., and Germany also begin relatively high in terms of income, but do not improve their position through time. Immigrants from Philippines begin on average below the median, but also greatly increase their relative position through time. On contrast, immigrants from Central America, Mexico, and Dominican Republic begin relatively low and do not greatly improve their position through time.

It is important to mention that the staying population of migrants is not necessarily the same as the group of permanent migrants. In strict terms, the evidence provided here refers to selectivity of long-term migrants while still active on their employment status. There is evidence in the literature that argues that later in life there is a peak of returning migration after retirement (Duleep, 1994; Steiner and Velling, 1994).

3.5.3 Recent selection patterns

Finally, an analysis of recent migration trends uses data from the 2000 census and the 2010 ACS to investigate if there are any patterns of recent return migration selection in terms of schooling. *Figures 3.7a* to *3.7j* present the results from this analysis. These figures illustrate the difference between the 2010 ACS and 2000 census histograms of a more detailed schooling attainment variable. The four panels presented in each figure vary by the population included in the analysis: (i) the first panel includes the whole sample, which is composed of individuals aged 25 to 54 in 2000 (35 to 64 in 2010) that report arriving to the U.S. between 1995 and 2000; (ii) the second panel includes only the male individuals form the first subsample; (iii) the third panel includes the younger individuals form the first subsample, aged 25 to 39 in 2000; and (iv) the fourth panel includes the older individuals from the first subsample, aged 40 to 54 in 2000.

A positive (negative) selection of immigrants that stay in the U.S. would result for a given

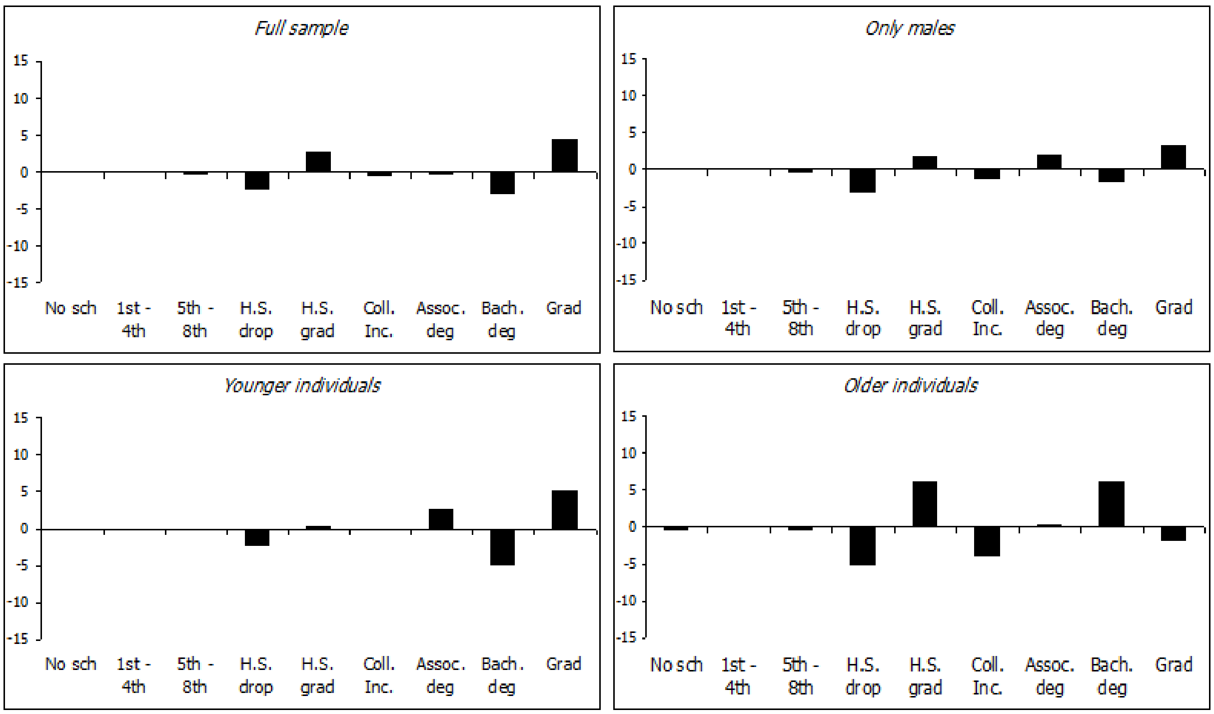


Figure 3.7a: Returners educational selectivity by gender and age groups. CANADA

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.

Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.

Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.

Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.

Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

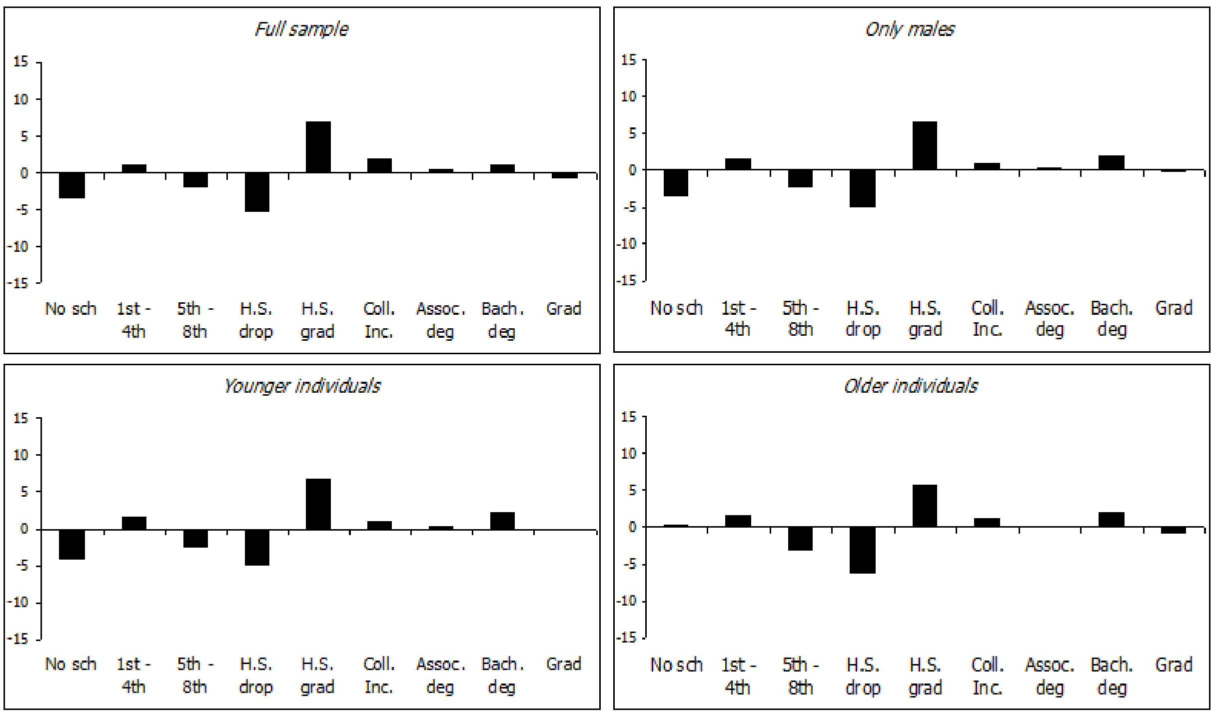


Figure 3.7b: Returners educational selectivity by gender and age groups. CENTRAL AMERICA

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.

Central America includes: Belize, Costa Rica, El Salvador, Guatemala, Honduras, Nicaragua, and Panama.

Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.

Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.

Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.

Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

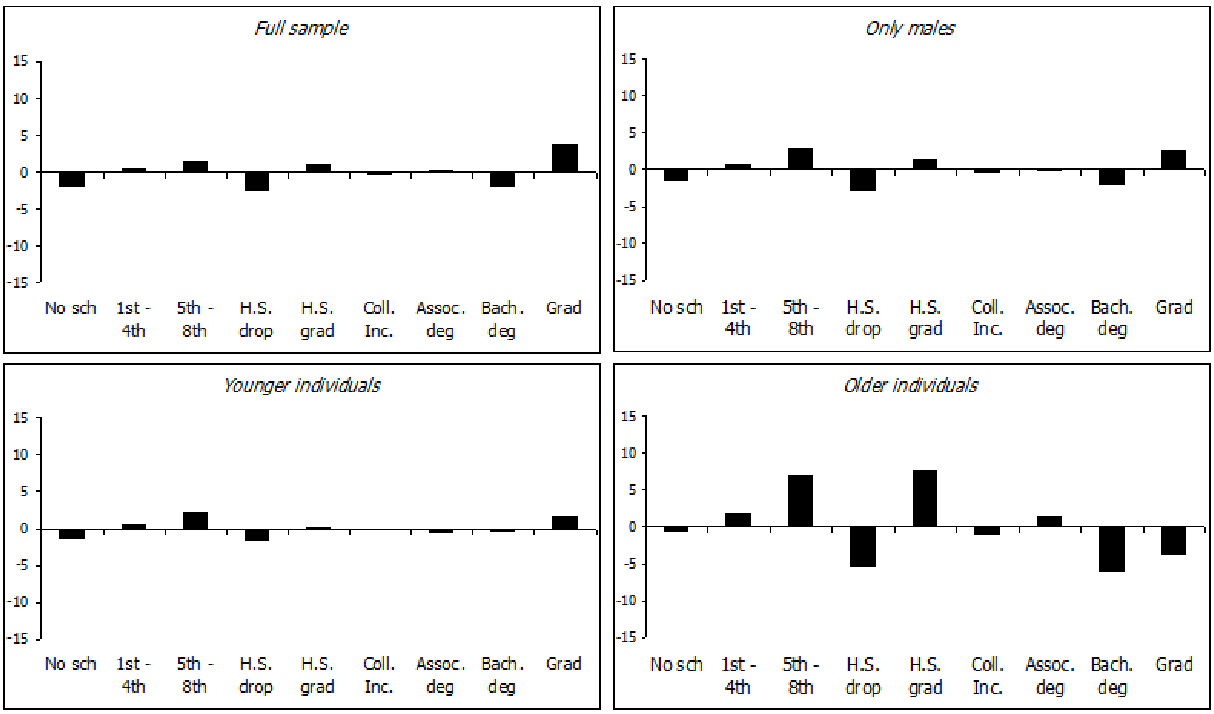


Figure 3.7c: Returners educational selectivity by gender and age groups. CHINA

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.

Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.

Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.

Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.

Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

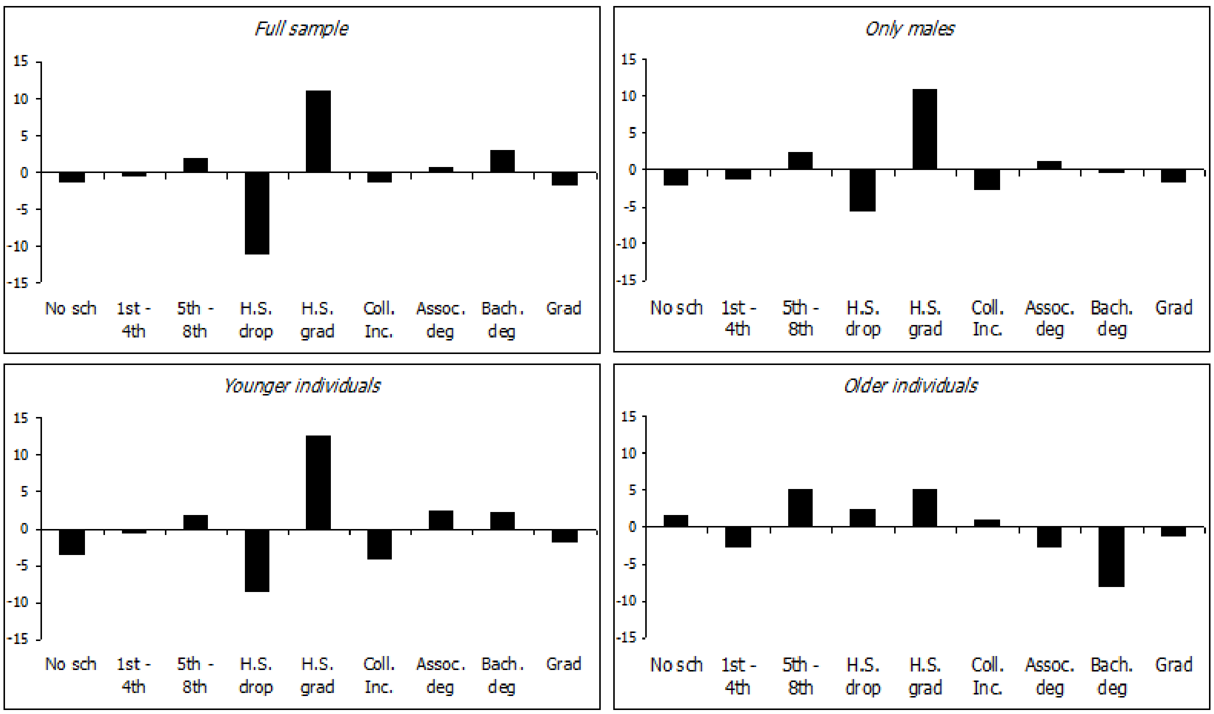


Figure 3.7d: Returners educational selectivity by gender and age groups. DOMINICAN REPULIC

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.

Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.

Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.

Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.

Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

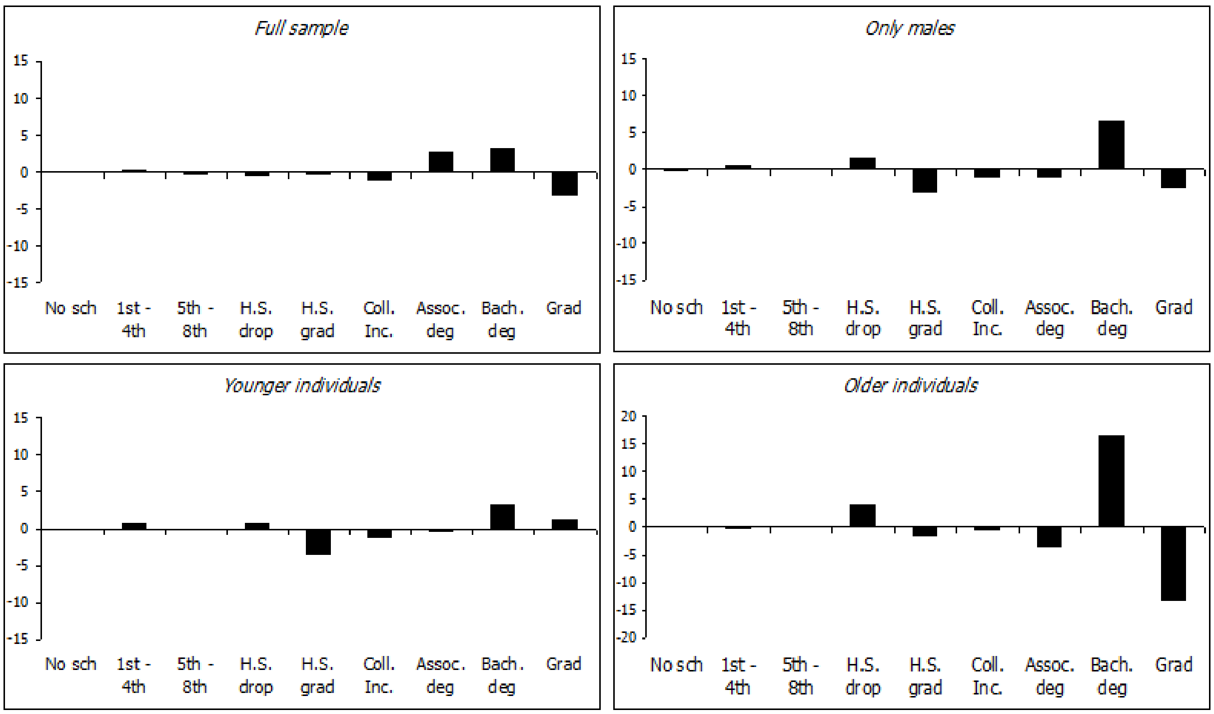


Figure 3.7e: Returners educational selectivity by gender and age groups. GERMANY

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.

Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.

Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.

Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.

Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

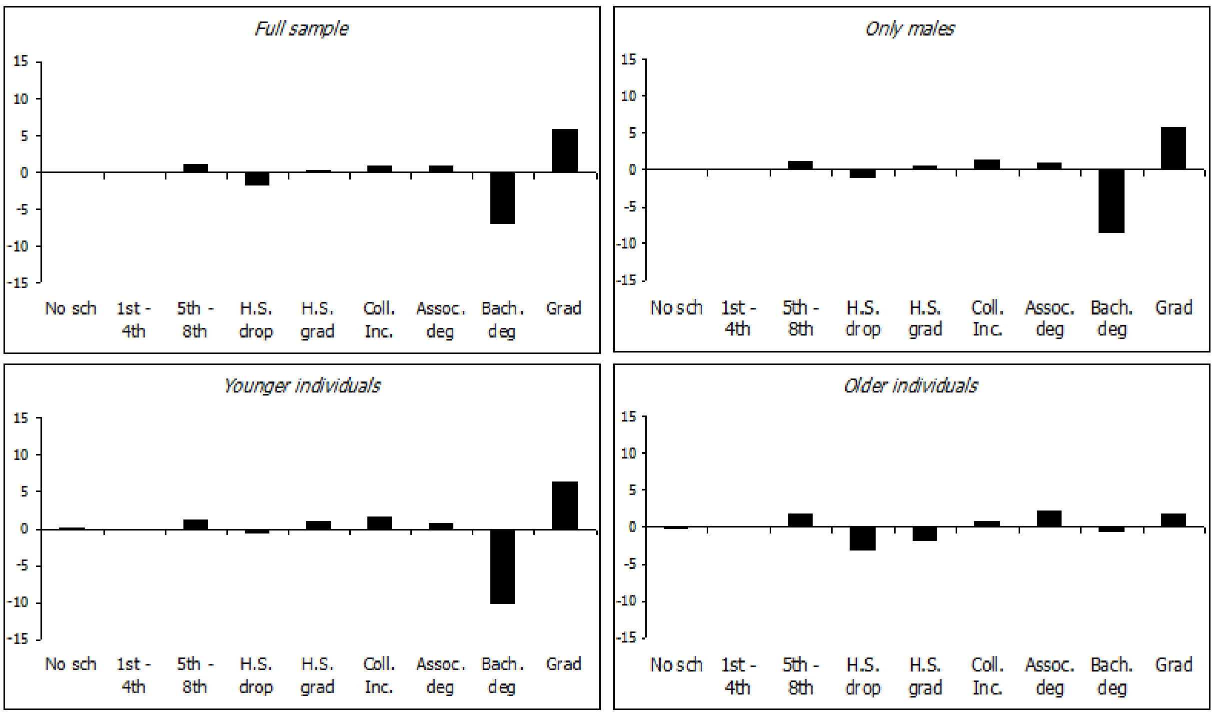


Figure 3.7f: Returners educational selectivity by gender and age groups. INDIA

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.

Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.

Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.

Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.

Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

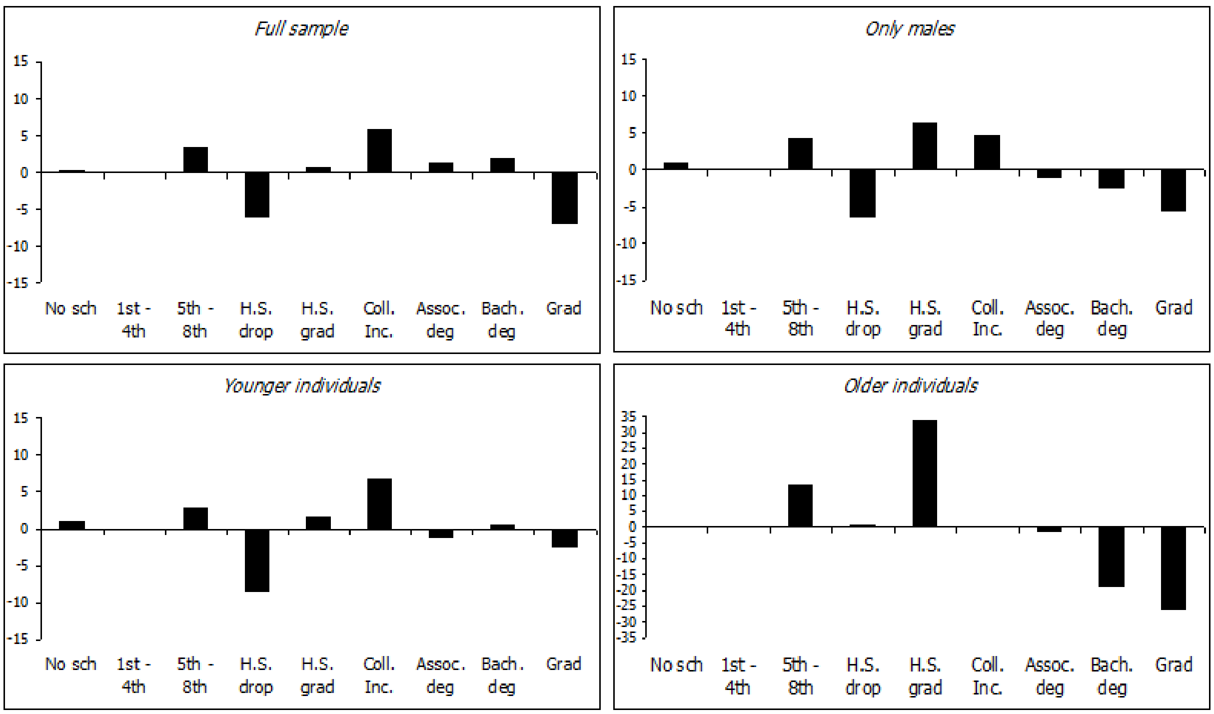


Figure 3.7g: Returners educational selectivity by gender and age groups. ITALY

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.

Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.

Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.

Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.

Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

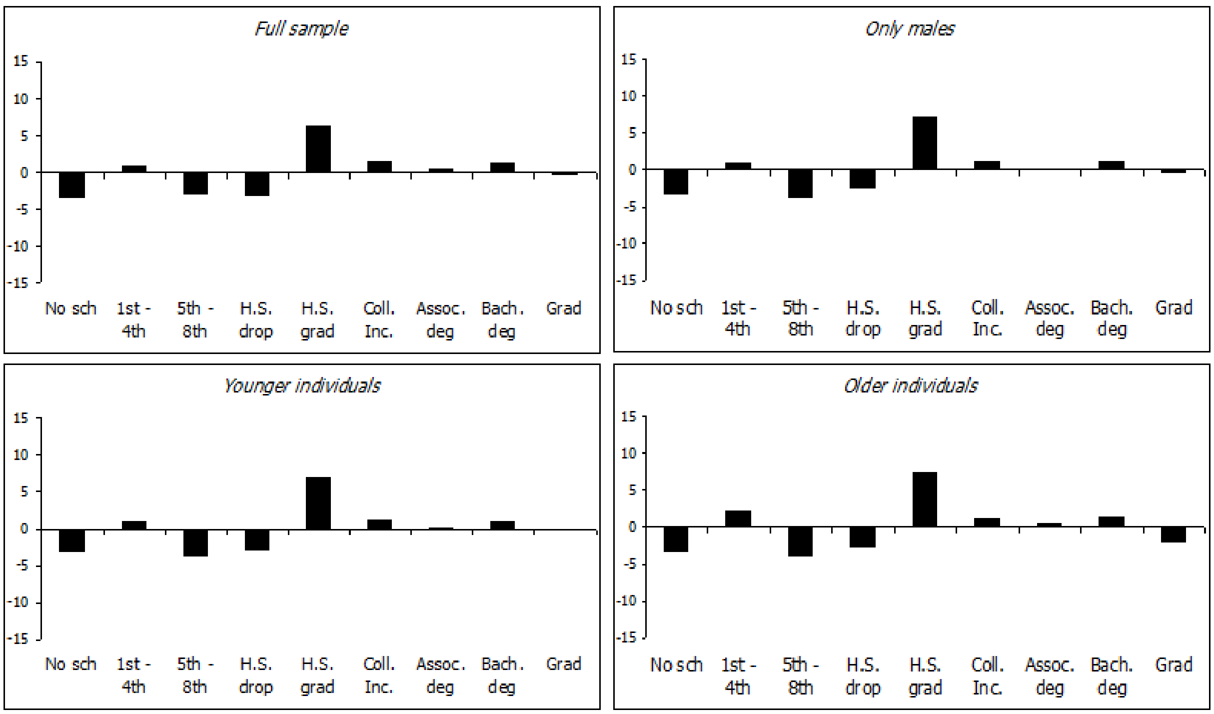


Figure 3.7h: Returners educational selectivity by gender and age groups. MEXICO

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.

Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.

Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.

Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.

Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

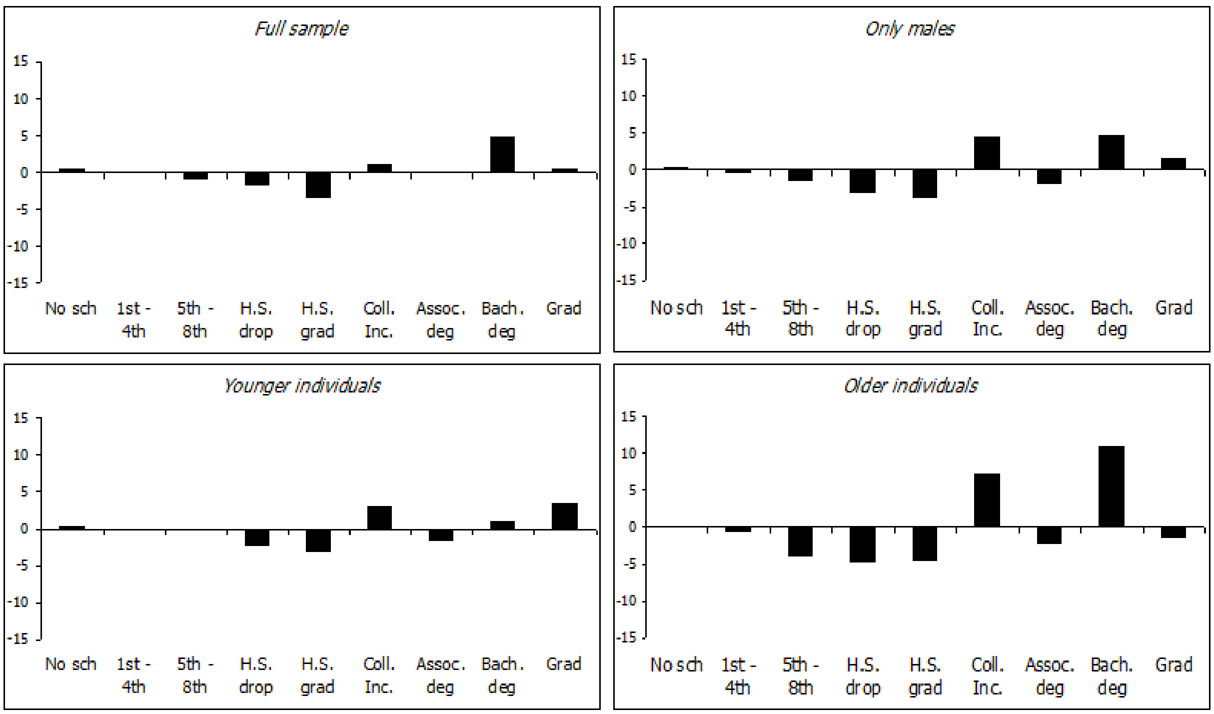


Figure 3.7i: Returners educational selectivity by gender and age groups. PHILIPPINES

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.

Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.

Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.

Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.

Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

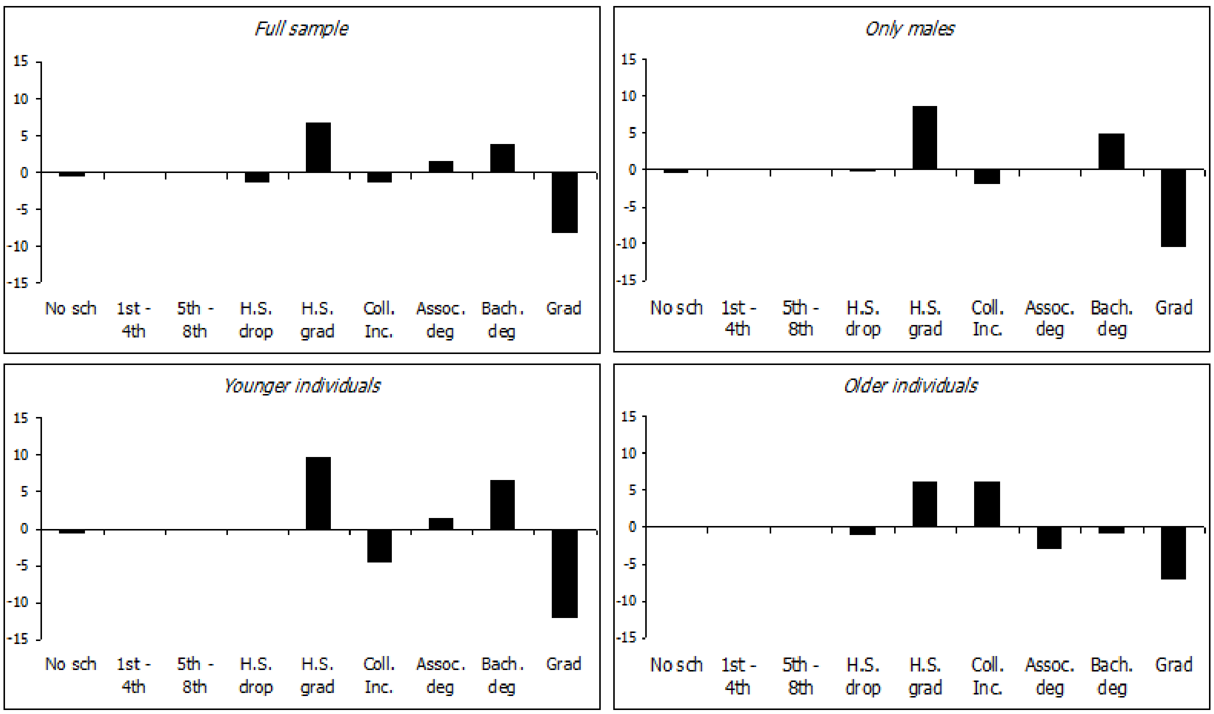


Figure 3.7j: Returners educational selectivity by gender and age groups. UNITED KINGDOM

The bars represent the difference between the 2010 and 2000 histograms at each schooling group. A positive (negative) value means that a higher (lower) proportion of the individuals had that level of schooling in 2010 than in 2000.
Full sample: Immigrants arrived 1995-2000, aged 25-54 in 2000.
Only males: Male immigrants arrived 1995-2000, aged 25-54 in 2000.
Younger individuals: Immigrants arrived 1995-2000, aged 25-39 in 2000.
Older individuals: Immigrants arrived 1995-2000, aged 40-54 in 2000.

subsample if there is a positive (negative) mass in the differences at higher levels of schooling and a negative (positive) mass at lower levels. In the case of positive selection, the positive mass at higher levels of schooling would mean that a higher proportion of the subsample had advanced school attainment in 2010 than in 2000.

Consistent with the analysis from the previous section, most of the country cases exhibit low differences between the 2010 and 2000 schooling distributions. Mexico, Central America, and Dominican Republic show similar selection patterns with positive selection of long-term migrants. This is mainly drawn from *High School* graduates being present in larger proportions in 2010, while *High School dropouts* and individuals with *no schooling* are present in lower proportions. In the case of the Dominican Republic, this trend results from the young individuals' pattern of selection. For Mexico and Central America, this pattern is consistently observed int he younger and older groups. India and Philippines also exhibit positive selection. In the case of India, it means that immigrants with *graduate level* studies stay in higher proportions while immigrants with *bachelor degree* studies leave. This result is driven from younger immigrants selectivity. Meanwhile, for Philippines the positive selection means immigrants with *bachelor* level studies and above, staying in higher proportions, while immigrants with *high school* level studies and below are present in smaller proportions in the follow-up.

Negative selection of long-term migrants is only observed for a few subsmaples. For example, older cohorts of immigrants form China, Germany, Italy, and U.K. see people with *bachelor* and *graduate level* studies in lower proportions in 2010 (only *graduate level* studies for Germany), while *high school graduates* and below tend to be present in larger proportions.

3.6 Conclusions

Ten countries were chosen based on their historical and present importance on U.S. migration. The empirical results shown on this paper suggest that there is overwhelming evidence in favor of positive selection of immigrants in terms of schooling, regardless of the source country's level of inequality and returns to schooling compared to the U.S. This positive selectivity has remained through time for most countries and in some cases it has even increased. In contrast, the analysis of return migration suggests that positive selectivity of staying migrants has decreased through time. The cohorts of migrants that arrived before 1980 exhibited positive selection of staying migrants in most of the countries analyzed. However, this positive selection is greatly reduced in later cohorts of migrants that arrived after 1995.

The case of Mexican migration to the U.S. has been the most thoroughly analyzed in the literature given the proportion of immigrants that these population represents. The evidence presented suggests that Mexican migration was positively selected during the 1970's and 1980's, but through time the positive selectivity disappeared. No evidence of negative selectivity was found though. With respect to return migration, the recent cohort suggests a slightly positive selection of migrants staying in the U.S. for a longer period.

Given that immigrants and returning migrants are not a random sample of a country's population, it is relevant to understand and track their patterns of selectivity. This is relevant from an economic and policy perspective, both for the source and receiving country. This has led to a growing literature that attempts to understand how the migration decision is taken and what components influence it.

APPENDIX

A. APPENDIX TO CHAPTER 1

Table A.1: Summary of Conley SEs.

						Peabody Test	Woodcock-Muñoz Test			Motor Skills		
	weight (lb) (1)	height (in) (2)	stunting (3)	anemia (4)	days_sick (5)	language (6)	long term memory (7)	short term memory (8)	visual-spatial thinking (9)	balance (seconds) (10)	ability to walk backward (11)	ability to walk straight (12)
coh97 x rain_shock	[0.6137] (0.6565) (0.6192)	[0.2190] (0.2629) 0.2881	.	[0.0624] (0.6) 0.0571	[0.2652] (0.2421) 0.2301	[0.0967] (0.1055) 0.12	[0.1042] (0.1199) 0.1187	[0.0257] (0.0288) 0.0287	[0.0424] (0.0496) 0.0516	[0.3420] (0.3686) 0.3064	[0.0162] (0.0186) 0.0208	[0.0117] (0.0109) 0.0097
coh98 x rain_shock	[0.4806] (0.5352) 0.6177	[0.2176] (0.2475) 0.2974	[0.0722] (0.0712) 0.0709	[0.0336] (0.0394) 0.0393	[0.1813] (0.1781) 0.1607	[0.0601] (0.0679) 0.0721	[0.0644] (0.0817) 0.0907	[0.0322] (0.0363) 0.0382	[0.0335] (0.0398) 0.0403	[0.2609] (0.2628) 0.2292	[0.0130] (0.0119) 0.0118	[0.0118] (0.0104) 0.0081
coh99 x rain_shock	[0.4399] (0.4924) 0.5619	[0.2483] (0.2826) 0.3272	[0.0672] (0.0720) 0.0806	[0.0368] (0.0364) 0.0343	[0.1860] (0.2170) 0.1896	[0.0682] (0.0651) 0.0612	[0.0598] (0.0649) 0.0735	[0.0370] (0.0357) 0.0345	[0.0493] (0.058) 0.0592	[0.3408] (0.3452) 0.3557	[0.0155] (0.0152) 0.0156	[0.0156] (0.0134) 0.0135
coh00 x rain_shock	[0.3484] (0.3378) 0.3627	[0.2485] (0.2514) 0.2852	[0.0630] (0.0658) 0.0725	[0.0338] (0.0368) 0.036	[0.2043] (0.2007) 0.1745	[0.0672] (0.0576) 0.046	[0.0568] (0.0565) 0.0502	[0.0374] (0.0356) 0.0381	[0.0541] (0.0486) 0.0423	[0.3739] (0.2811) 0.2735	[0.0283] (0.0246) 0.0236	[0.0374] (0.0351) 0.0353
coh01 x rain_shock	[0.3742] (0.3991) 0.3959	[0.2052] (0.1867) 0.1946	[0.0653] (0.073) 0.0844	[0.0616] (0.0716) 0.0751	[0.2517] (0.2211) 0.1998	[0.6933] (0.6910) 0.6822	[0.0549] (0.066) 0.0651	[0.0552] (0.0493) 0.0388	[0.0899] (0.1003) 0.0944	[0.4352] (0.3796) 0.3306	[0.0563] (0502) 0.0432	[0.0509] (0.0488) 0.0536

Robust standard errors clustered by pixel are reported in brackets; Conley SEs, with cutoff=1decimal deg, are reported in parentheses; and the remaining figures are Conley standard errors with cutoff=2 decimal deg.

Table A.2: Robustness check: Effect of the 1999 September-October rainfall shock on anthropometric and health outcomes controlling for variables that show significant differences in the exogeneity test

	weight (lb)	height (in)	Stunting	Anemia	Days_sick
	(1)	(2)	(3)	(4)	(5)
coh97 x rain_shock	-0.497	-0.448**	.	-0.0369	0.403
	[0.6128]	[0.2179]	.	[0.0614]	[0.2723]
coh98 x rain_shock	-0.715	-0.687***	0.129*	-0.0160	0.0306
	[0.4672]	[0.2067]	[0.0711]	[0.0332]	[0.1789]
coh99 x rain_shock	-0.651	-0.507**	0.126**	-0.000984	-0.222
	[0.4071]	[0.2297]	[0.0629]	[0.0362]	[0.1749]
coh00 x rain_shock	-0.103	-0.401*	0.135**	0.00612	0.0250
	[0.3290]	[0.2381]	[0.0598]	[0.0338]	[0.2064]
coh01 x rain_shock	-0.357	-0.140	0.103	0.00328	0.127
	[0.3509]	[0.1873]	[0.0619]	[0.0615]	[0.2529]
Observations	3730	3706	2776	3764	3376
R^2	0.58	0.76	0.10	0.03	0.02
Mean	33.42	38.10	0.384	0.273	1.282

rain_shock=1 if child was present in a village with a flood occurrence in 1999
Standard errors clustered by gridcell [in brackets]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Additional controls: TV and vehicle ownership, chickens owned, permanent migrants to the U.S.

Table A.3: Robustness check: Effect of the 1999 September-October rainfall shock on cognitive development outcomes controlling for variables that show significant differences in the exogeneity test

	Peabody Test	**Woodcock-Muñoz Test**		
	language	long term memory	short term memory	visual-spatial thinking
	(1)	(2)	(3)	(4)
coh97 x rain_shock	-0.199**	-0.175*	0.00643	-0.123***
	[0.0963]	[0.1015]	[0.0252]	[0.0420]
coh98 x rain_shock	-0.196***	-0.189***	0.0185	-0.115***
	[0.0566]	[0.0605]	[0.0312]	[0.0307]
coh99 x rain_shock	-0.125*	-0.141**	-0.0467	-0.101**
	[0.0673]	[0.0580]	[0.0364]	[0.0459]
coh00 x rain_shock	-0.00328	-0.133**	0.0223	-0.132**
	[0.0654]	[0.0575]	[0.0368]	[0.0519]
coh01 x rain_shock	-0.225	-0.0168	-0.0686	0.0489
	[0.6753]	[0.0524]	[0.0552]	[0.0911]
Observations	2840	3521	3384	2966
R^2	0.34	0.32	0.48	0.39

rain_shock=1 if child was present in a village with a flood occurrence in 1999
Standard errors clustered by gridcell [in brackets]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$
Additional controls: TV and vehicle ownership, chickens owned, permanent migrants to the U.S

Table A.4: Robustness check: Effect of the 1999 September-October rainfall shock on gross motor skill outcomes controlling for variables that show significant differences in the exogeneity test

	balance (seconds)	ability to walk backward	ability to walk straight
	(1)	(2)	(3)
coh97 x rain_shock	-0.646*	0.0264	0.0154
	[0.3285]	[0.0162]	[0.0120]
coh98 x rain_shock	-0.124	0.0154	0.0235*
	[0.2588]	[0.0132]	[0.0123]
coh99 x rain_shock	-0.289	0.0186	-0.0343**
	[0.3319]	[0.0155]	[0.0155]
coh00 x rain_shock	-0.169	-0.0194	-0.00535
	[0.3763]	[0.0287]	[0.0378]
coh01 x rain_shock	0.157	-0.0295	-0.0486
	[0.4289]	[0.0561]	[0.0514]
Observations	3562	3692	3662
R^2	0.33	0.16	0.24
Mean	8.286	0.891	0.843

rain_shock=1 if child was present in a village with a flood occurrence in 1999
Standard errors clustered by gridcell [in brackets]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Additional controls: TV and vehicle ownership, chickens owned, permanent migrants to the U.S

Table A.5: Sensitivity tests: analysis presented in Table 1.3 are reproduced here using rainfall variables corresponding to two different cutoff points (standardized precipitation anomalies higher than 1 and 0.5 standard deviations, respectively). As in Table 1.3, this table presents the effect of the 1999 September-October rainfall shock on anthropometric indicators measured in 2003 for children born between 1997 and 2001: weight measured in pounds; height measured in inches, stunting, anemia, and number of sick days.

	weight (lb)[a]	height (in)[a]	stunting[b]	anemia[c]	days_sick[d]
	(1)	(2)	(3)	(4)	(5)
rain_shock= 1 if standardized precipitation anomaly > 0.5					
coh97 x rain_shock[e]	-1.823***	-0.787***	.	0.0615	0.256
	[0.6116]	[0.2294]	.	[0.0604]	[0.2864]
coh98 x rain_shock	-0.932*	-0.718***	0.113	0.0276	0.00733
	[0.5041]	[0.2208]	[0.0752]	[0.0355]	[0.1989]
coh99 x rain_shock	-0.826*	-0.545**	0.121*	0.0263	-0.0961
	[0.4732]	[0.2507]	[0.0664]	[0.0378]	[0.2180]
coh00 x rain_shock	-0.527	-0.454*	0.118*	0.00320	0.307
	[0.3765]	[0.2433]	[0.0635]	[0.0438]	[0.2142]
coh01 x rain_shock	-0.494	-0.288	0.142**	0.0772	0.100
	[0.3694]	[0.2118]	[0.0659]	[0.0673]	[0.2418]
Observations	3729	3705	2777	3765	3377
R^2	0.58	0.76	0.08	0.03	0.02
Mean	33.42	38.08	0.384	0.273	1.282
rain_shock= 1 if standardized precipitation anomaly > 1					
coh97 x rain_shock[e]	-0.838	-0.181	.	0.0244	0.0832
	[0.5606]	[0.2299]	.	[0.0580]	[0.2725]
coh98 x rain_shock	0.137	-0.0443	0.0229	-0.00490	-0.0138
	[0.5236]	[0.2706]	[0.0781]	[0.0317]	[0.1619]
coh99 x rain_shock	0.241	0.295	-0.0685	0.00160	-0.109
	[0.4640]	[0.2963]	[0.0808]	[0.0363]	[0.1606]
coh00 x rain_shock	0.436	0.249	-0.0309	-0.00599	0.117
	[0.3211]	[0.2736]	[0.0722]	[0.0315]	[0.1738]
coh01 x rain_shock	0.0869	0.230	-0.0489	-0.0719	0.0667
	[0.3684]	[0.2221]	[0.0747]	[0.0561]	[0.2524]
Observations	3729	3705	2777	3765	3377
R^2	0.57	0.75	0.08	0.03	0.02
Mean	33.42	38.08	0.384	0.273	1.282

Standard errors clustered by gridcell [in brackets]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[a] Weight and height are measures in pounds and inches respectively and are normalized by age.

[b] Stunting is a binary variable = 1 if the child is stunted. Stunting is defined as being two or more standard deviations below the age-sex standardized height of a healthy reference population (World Health Organization, 1996).

[c] Anemia is a binary variable = 1 if the child is anemic. Anemia is defined as hemoglobin less than 11 g/dL adjusted for altitude using standard adjustments (Ruiz-Argüelles and Llorente-Peters, 1981).

[d] Number of days in the previous 4 weeks that the child was reported sick by the mother.

[e] *coh97 x rain_shock* indicates the interaction between the variable *coh97* (=1 if the child was born in 1997) and the variable *rain_shock* (=1 if a rainfall shock occurred in 1999).

Table A.6: Sensitivity tests: analysis presented in Table 1.4 are reproduced using rainfall variables corresponding to two different cutoff points (standardized precipitation anomalies higher than 1 and 0.5 standard deviations respectively). As in Table 1.4, this table presents estimates the effect of the 1999 September-October rainfall shock on cognitive development indicators measured in 2003 for children born between 1997 and 2001: Peabody Test Scores (i.e. language abilities), and Woodcokc-Muñoz test Scores (i.e. long-term memory, short term memory, and visual-spatial thinking. Test scores are measured in logs.

	Peabody Test[a]		**Woodcock-Muñoz Test**[b]	
	language	long term memory	short term memory	visual-spatial thinking
	(1)	(2)	(3)	(4)
rain_shock= 1 if standardized precipitation anomaly > 0.5				
coh97 x rain_shock[c]	-0.267***	-0.254***	-0.0113	-0.136***
	[0.0975]	[0.0872]	[0.0245]	[0.0403]
coh98 x rain_shock	-0.176**	-0.216***	-0.00883	-0.117***
	[0.0833]	[0.0611]	[0.0255]	[0.0311]
coh99 x rain_shock	-0.0776	-0.160**	-0.0580	-0.138***
	[0.0822]	[0.0692]	[0.0400]	[0.0481]
coh00 x rain_shock	0.000209	-0.150**	-0.0132	-0.149***
	[0.0728]	[0.0619]	[0.0391]	[0.0517]
coh01 x rain_shock	.	-0.0547	-0.0825	0.135
	.	[0.0499]	[0.0552]	[0.0974]
Observations	2835	3522	3385	2967
R^2	0.33	0.32	0.48	0.38
rain_shock= 1 if standardized precipitation anomaly > 1				
coh97 x rain_shock[c]	-0.0872	-0.0997	0.0193	-0.0863*
	[0.1018]	[0.1001]	[0.0242]	[0.0453]
coh98 x rain_shock	-0.0910	-0.137*	-0.0203	-0.0589
	[0.0680]	[0.0742]	[0.0273]	[0.0417]
coh99 x rain_shock	0.0225	0.00479	0.0263	-0.00585
	[0.0762]	[0.0654]	[0.0421]	[0.0587]
coh00 x rain_shock	0.00675	-0.0499	0.0386	-0.0781
	[0.0646]	[0.0660]	[0.0343]	[0.0575]
coh01 x rain_shock	-0.221	0.0179	-0.0430	0.0866
	[0.7175]	[0.0570]	[0.0543]	[0.0758]
Observations	2835	3522	3385	2967
R^2	0.33	0.31	0.48	0.38

Standard errors clustered by gridcell [in brackets]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[a] Peabody Test measures verbal abilities, scores are standardized by age. Peabody test scores are a reliable predictor of achievements in primary school.

[b] Woodcock-Muñoz Test is used to assess a wide range of cognitive abilities: long-term memory, short-term memory and visual spatial thinking.

[c] *coh97 x rain_shock* indicates the interaction between the variable *coh97* (=1 if the child was born in 1997) and the variable *rain_shock* (=1 if a rainfall shock occurred in 1999).

Table A.7: Sensitivity tests: analysis presented in Table 1.5 are reproduced using rainfall variables corresponding to two different cutoff points (standardized precipitation anomalies higher than 1 and 0.5 standard deviations respectively). As in Table 1.5, this table shows the effect of the 1999 September-October rainfall shock on gross motor skills measured in 2003 for children born between 1997 and 2001: ability to keep their balance on one foot (measured in seconds), ability to work forward and backward. These gross motor skills are central to the successful performance of school tasks.

	balance on one foot (seconds)[a]	ability to walk backward[b]	ability to walk straight[b]
	(1)	(2)	(3)
rain_shock= 1 if standardized precipitation anomaly> 0.5			
coh97 x rain_shock[c]	-0.471	0.0392**	0.00646
	[0.3474]	[0.0186]	[0.0130]
coh98 x rain_shock	0.143	0.0210	0.0234*
	[0.2689]	[0.0136]	[0.0135]
coh99 x rain_shock	-0.356	0.0160	-0.0377**
	[0.3647]	[0.0165]	[0.0175]
coh00 x rain_shock	-0.180	-0.0186	-0.0112
	[0.3692]	[0.0333]	[0.0414]
coh01 x rain_shock	0.162	-0.0664	0.0653
	[0.4701]	[0.0471]	[0.0537]
Observations	3563	3693	3663
R^2	0.33	0.16	0.24
Mean	8.286	0.891	0.843
rain_shock= 1 if standardized precipitation anomaly> 1			
coh97 x rain_shock[c]	-0.242	0.0294**	0.00733
	[0.3547]	[0.0146]	[0.0114]
coh98 x rain_shock	0.0606	0.0107	0.0211*
	[0.2620]	[0.0122]	[0.0107]
coh99 x rain_shock	0.304	0.0314*	-0.0103
	[0.3564]	[0.0164]	[0.0172]
coh00 x rain_shock	0.192	-0.0210	-0.0309
	[0.3696]	[0.0309]	[0.0379]
coh01 x rain_shock	0.390	-0.00394	-0.0245
	[0.3915]	[0.0569]	[0.0415]
Observations	3563	3693	3663
R^2	0.33	0.16	0.24
Mean	8.286	0.891	0.843

Controlling for household and individual characteristics.
Standard errors clustered by gridcell [in brackets]. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

[a] The child's ability keep her/his balance on one foot is measured in seconds.

[b] The binary variables = 1 if the child was able to successfully complete the task.

[c] *coh97 x rain_shock* indicates the interaction between the variable *coh97* (=1 if the child was born in 1997) and the variable *rain_shock* (=1 if a rainfall shock occurred in 1999).

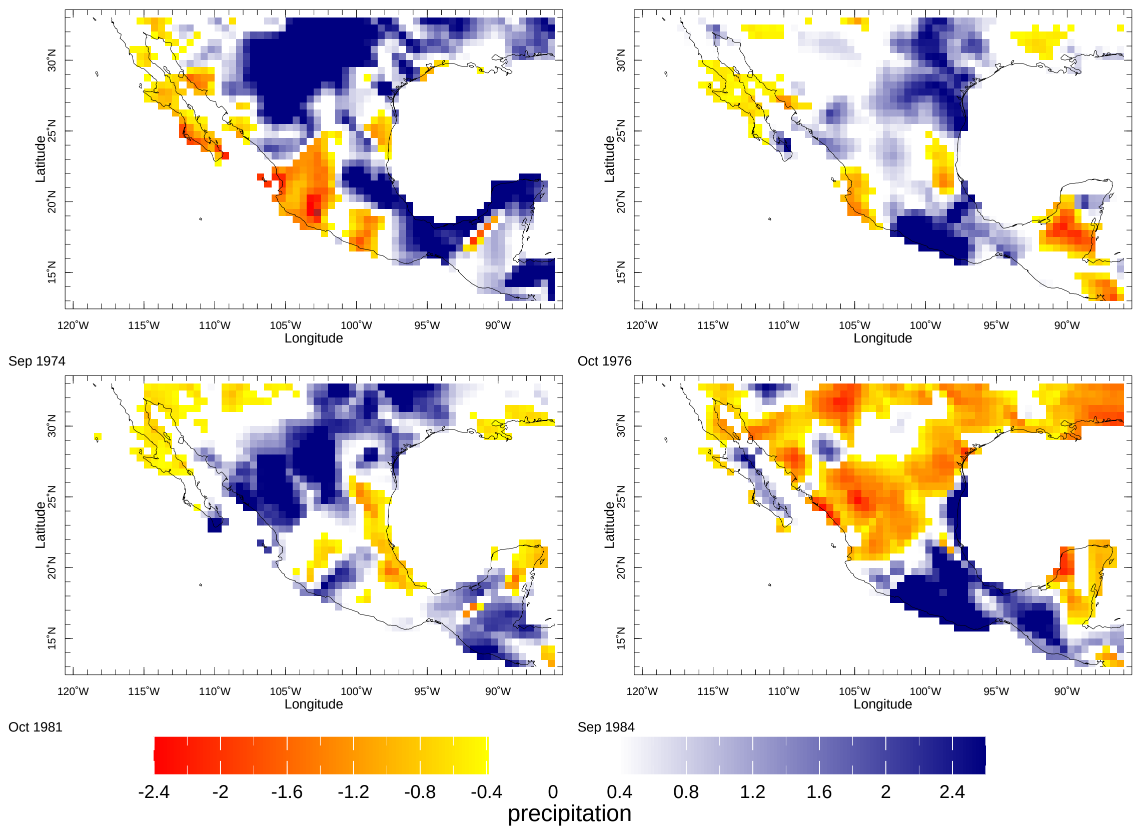


Figure A.1: ENSO historical. Precipitation Standardized Anomalies (UEA CRU Ts2p1)

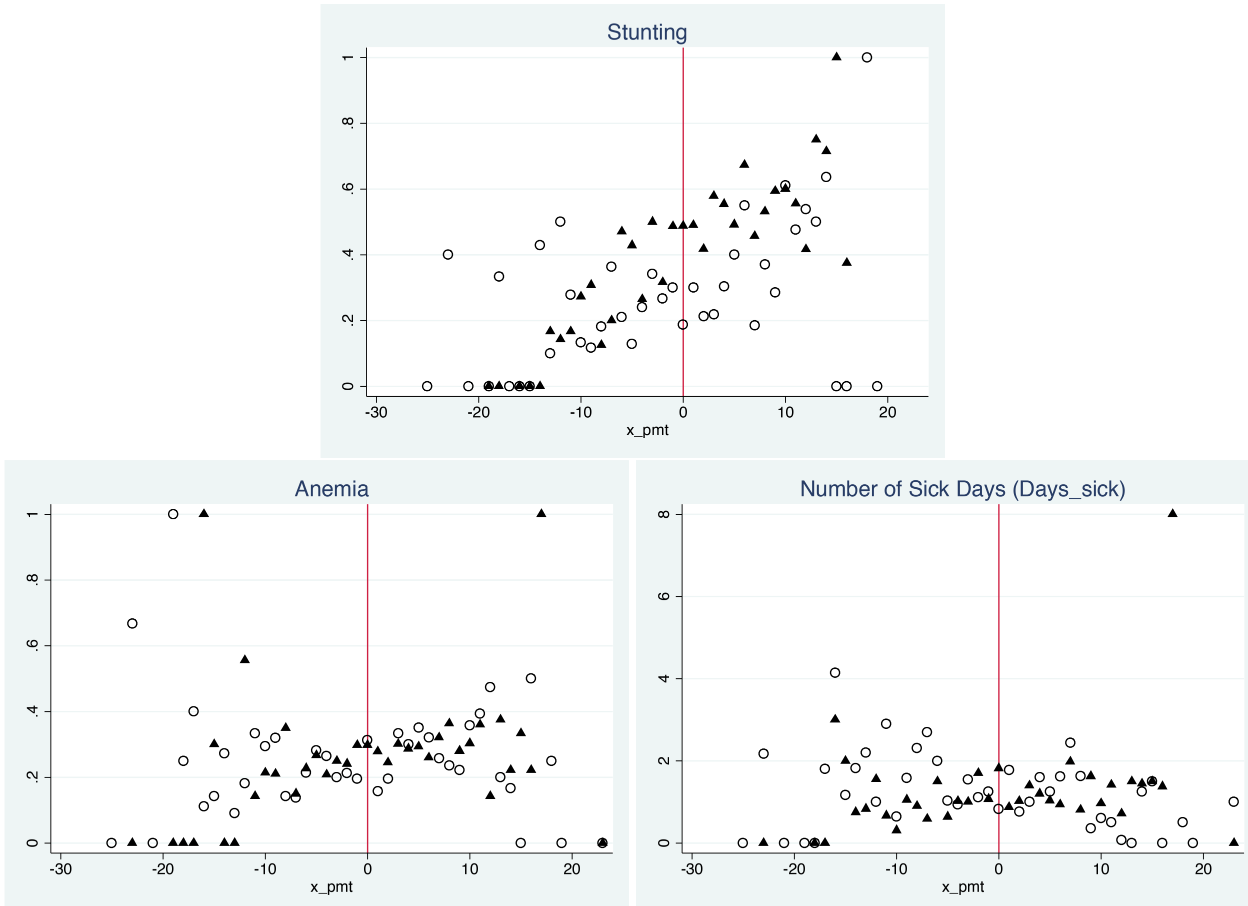


Figure A.2: RD analysis: Anthropometric Outcomes

On each graph, the x-axis corresponds to the standardized poverty index used by the administrative rule to select *Progresa* beneficiaries. The administrative cutoff is centered at zero.
The standardized poverty index (*x_pmt*) is formed with a formula that weights household's asset ownership and socio-economic characteristics of its members.
Analysis restricted to original randomized treatment villages.
The y-axis gives conditional means of the individual outcomes. ▲is the conditional mean for individuals from villages affected by a rain shock. ○is the conditional mean for individuals from villages not affected by a rain shock.

Figure A.3: RD analysis: Cognitive Outcomes

On each graph, the x-axis corresponds to the standardized poverty index used by the administrative rule to select *Progresa* beneficiaries. The administrative cutoff is centered at zero.
The standardized poverty index (*x_pmt*) is formed with a formula that weights household's asset ownership and socio-economic characteristics of its members.
Analysis restricted to original randomized treatment villages.
The y-axis gives conditional means of the individual outcomes. ▲is the conditional mean for individuals from villages affected by a rain shock. ○is the conditional mean for individuals from villages not affected by a rain shock.

B. APPENDIX TO CHAPTER 3

Table B.1: Number of observations for each country by year and cohort

Cohort	Year	Country									
		CAM	CAN	CHI	D.R.	ENG	GER	IND	ITA	MEX	PHI
1	1970	73	152	95	47	149	117	88	183	216	135
1	1980	447	559	596	344	502	454	345	895	1,558	844
1	1990	346	444	463	198	397	436	288	635	1,229	797
2	1980	767	556	698	340	610	328	1,009	284	3,508	1,352
2	1990	943	444	762	239	436	254	948	233	3,605	1,604
2	2000	1,089	446	805	272	473	293	1,024	215	3,824	1,530
3	1990	2,650	695	1,111	552	591	417	1,183	176	5,430	2,306
3	2000	3,312	763	2,481	943	536	453	1,678	183	7,797	3,132
3	2010	666	142	531	191	81	73	419	32	1,683	753
4	2000	2,496	1,605	2,071	748	768	1,181	2,693	269	11,882	1,661
4	2010	706	337	948	237	125	176	907	65	2,930	626

Source: U.S. 1970, 1980, 1990, and 2000 Census and 2010 American Community Survey

Table B.2: Education variables standardization

IPUMS code	IPUMS Definition	Availability 1970	1980	1990	2000	2010	Barro and Lee standardization	Sch_var standardization
0	N/A or no schooling				X		.	.
1	N/A	X	X	X		X	.	.
2	No schooling	X	X	X	X	X	No school	No school
10	Nursery to 4th		X				Prim. inc.	Grade 1-4
11	Nursery - preschool	X		X	X	X	Prim. inc.	Grade 1-4
12	Kinder	X		X	X	X	Prim. inc.	Grade 1-4
13	Grade 1st-4th			X			Prim. inc.	Grade 1-4
14	Grade 1	X			X	X	Prim. inc.	Grade 1-4
15	Grade 2	X			X	X	Prim. inc.	Grade 1-4
16	Grade 3	X			X	X	Prim. inc.	Grade 1-4
17	Grade 4	X			X	X	Prim. inc.	Grade 1-4
20	Grade 5th-8th			X			Prim. comp.	Grade 5-8
21	Grade 5th-6th		X				Prim. comp.	Grade 5-8
22	Grade 5	X			X	X	Prim. comp.	Grade 5-8
23	Grade 6	X			X	X	Prim. comp.	Grade 5-8
24	Grade 7th-8th		X				Prim. comp.	Grade 5-8
25	Grade 7	X			X	X	Prim. comp.	Grade 5-8
26	Grade 8	X			X	X	Prim. comp.	Grade 5-8
30	Grade 9	X	X	X	X	X	Sec. inc	H.S. inc.
40	Grade 10	X	X	X	X	X	Sec. inc	H.S. inc.
50	Grade 11	X	X	X	X	X	Sec. inc	H.S. inc.
60	Grade 12				X	X	Sec. comp.	
61	Grade 12- no diploma	X	X	X			Sec. comp.	H.S. inc.
62	HS degree or GED		X	X			Sec. comp.	H.S. grad.
63	HS degree	X					Sec. comp.	H.S. grad.
64	GED	X					Sec. comp.	H.S. grad.
65	Some college: ¡1 yr	X	X		X	X	Sec. comp.	H.S. grad.
70	College: 1+ yrs				X	X	Tert. inc.	
71	College: 1+yrs no degree	X	X	X			Tert. inc.	Some coll.
80	College: 2 yrs				X	X	Tert. inc.	
81	Associate deg: no spec	X	X				Tert. inc.	Assoc. deg.
82	Associate deg: occup			X			Tert. inc.	Assoc. deg.

Continued on next page

Table B.2 – continued

IPUMS		Availability					Barro and Lee	Sch_var
code	IPUMS Definition	1970	1980	1990	2000	2010	standardization	standardization
83	Associate deg: academic			X			Tert. inc.	Assoc. deg.
90	College: 3 yrs				X	X	Tert. inc.	
100	College: 4 yrs				X	X	Tert. inc.	
101	Bachelor deg	X	X	X			Tert. comp.	Bach. deg.
110	College: 5+ yrs				X	X	Tert. comp.	
111	College: 6 yrs				X	X	Tert. comp.	
112	College: 7 yrs				X		Tert. comp.	
113	College: 8+ yrs				X		Tert. comp.	
114	Masters deg	X	X	X			Tert. comp.	Grad. deg.
115	Professional deg	X	X	X			Tert. comp.	Grad. deg.
116	Doctoral deg	X	X	X			Tert. comp.	Grad. deg.

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